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LINGUISTIC STUDIES OFFERED TO JOSEPH GREENBERG

edited by Alphonse Juilland

THIRD VOLUME: SYNTAX

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LINGUISTIC STUDIES
OFFERED TO
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STUDIA LINGUISTICA ET PHILOLOGICA

editor

ALPHONSE JUILLAND

Stanford University

VOLUME 4

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ALPHONSE JUILLAND

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1976
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LC 76-16895
ISBN 0-915838-43-5
Printed in the United States of America.

THIRD VOLUME

SYNTAX

Iosepho Greenberg,
viro imprimis docto atque humano, qui proprietates omnium linguarum communes summo studio, perspicacitate eximia penitus indagavit, hunc librum dedicamus
amici, discipuli, collegae

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Universal Categories: On the Semantics of Classifiers and Children's early Word Meanings*

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Introduction

Classifiers are words or particles used in classifying things. In English, for example, the words *head* and *sheet* in *200 head of cattle* and *50 sheets of paper* act rather like classifiers: they serve to individuate the entities that together make up the herd of cattle or the pile of paper. In many languages, classifiers like this are used with numeral-noun combinations or with certain types of verbs. When one looks at the semantics of classifiers in natural languages, one finds that certain properties of the entities referred to are 'picked out' as criteria for category membership. Some of these properties are primary and can be used as the sole basis for the classification; others are secondary and normally occur in combination with some primary property. Many of the properties that form the basis for such classifications depend on our visual perception of objects in the world.

Children use words to classify their surroundings too as they begin to map language onto what they already know about the world. In the process of acquisition, though, they sometimes come up with categories that do not quite coincide with the adult's. And the basis for categorization discernible in the child's early uses of words often bears a strong resemblance to that found in actual classifier systems. In both, visual perception appears to play a major role in determing category membership.

In the present paper, I shall examine the resemblance between the

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* The preparation of this paper was supported in part by NSF GS-30040 and in part by NSF SO75-17126. I am grateful to Herbert H. Clark, Joseph H. Greenberg, and Paul Kay for their comments on an earlier version of this paper that was presented at the 73rd Annual Meeting of the American Anthropological Association, Mexico City, in November 1974.

semantics of classifier systems and the child's emerging knowledge about word meanings, and I shall argue that the categories identified by classifiers and by children are a reflection of certain basic cognitive capacities. The properties chosen as criterial for category membership by young children are presumably those that are most salient for the young child. The same properties appear to govern numeral and verbal classifier systems. I will suggest that these natural categories may be universal precisely because they have a common cognitive basis.

Classification in natural languages

Numeral classifiers and verbal classifiers are among a number of semantically based classification systems found in natural languages. For the most part, linguistic analysis has focussed on their syntactic properties and on their relations to quantification with numerals (e.g. Greenberg 1972). However, some ethnographers have made detailed investigations into the basis for usage of different classifiers in languages like Tzeltal (Berlin and Romney 1964; Berlin 1968). There have also been several studies of verbal classifiers in languages like Navaho, Hopi, and Atsugewi (e.g. Hoijer 1951; Carroll and Casagrande 1958; Talmy 1972). More recently, still, Adams and Conklin (1973) made an extensive survey of the semantics of numeral classifier systems for some 37 Asian languages drawn from the Malayo-Polynesian, Austro-Asiatic, Mon-Khmer, Sino-Tibetan, Altaic, Dravidian, and Indo-Aryan language families. The number of classifiers used in each of the languages surveyed ranges from two up to about 200.

How exactly is a classifier used? Consider the following expressions from English:

(1) a. 25 sheets/leaves of paper
b. 4 strings of beads
c. 9 cups of tea

The nouns *sheet* (or *leaf*), *string*, and *cup* in these English phrases pick out the individual entities that make up each 'grouping'. In languages that use numeral classifiers, this form of classification can extend to all entities that are countable. Some typical examples have been listed in (2):

(2) a. 7 flat-things rugs
b. 16 long-things poles
c. 3 round-things gourds
d. 5 long & flexible-things ropes

In each case, the classifier construction serves to pick out the individual entities on the basis of some observable property and then names the class they belong to. Thus, (2a) could be glossed as *7 flat-things (that are) rugs* (see Greenberg 1972). The properties *long, round,* and *flat* are often the only properties picked out by the classifiers. However, they can also be combined with other, secondary properties, as in (2d) where *long* is combined with *flexible* (see Adams and Conklin 1973).

Examples of classifiers that form part of the verb are listed in (3):

(3) a. he caused-round-solid-thing-to-move-upwards stone ('he picked up the stone')
b. wood moves-about-hooplike (nominal: 'wagon')
c. the man animate-thing-moved-away ('the man moved away/left')

The classifier 'themes' attached to the verb stem in Navaho, for example, draw the same kinds of distinctions as those found in numeral classifier systems. Navaho verbs for handling objects can choose from the themes shown in (4), depending on what the object is that gets moved (Hoijer 1951):

(4) a. animate-object
b. round-solid-object; long-slender-object; rope-like-object; rigid-container; fabric-like-object
c. set- or aggregate-of-objects; set-of-parallel-objects
d. bulky-object; unspecified-mass; wool-like-mass; mud-like-mass

The four groups of classifiers in (4) are very common in both numeral and verbal classifier systems among American Indian languages (e.g. Hoijer 1945; Sapir 1951; Davidson, Elford and Hoijer 1963; Krauss 1965). These sub-classes of classifiers all seem to fit into the general framework proposed by Adams and Conklin (1973) in their analysis of Asian classifier languages.

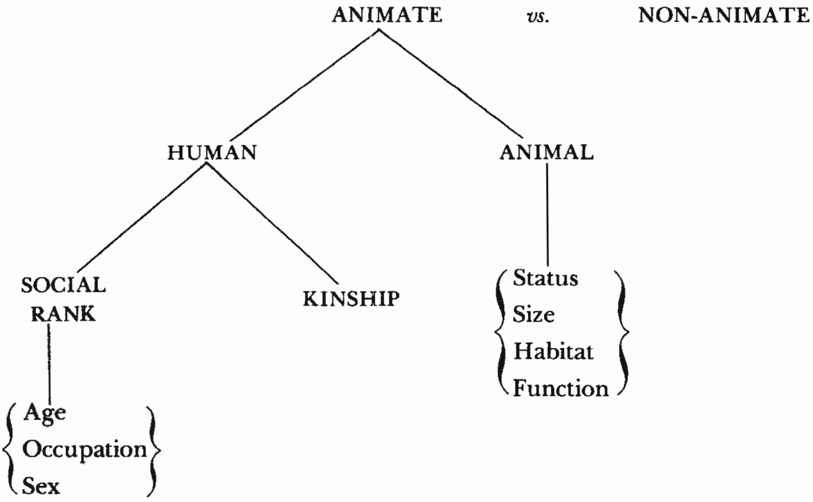
The primary distinction classifier systems usually mark is one between animate and non-animate entities. The Animate entities are subdivided into Human versus Animal, and Human may be still further subdivided by social rank, or less commonly, by kinship. These sub-classifications generally depend on age (the most common), occupation, and sex (less common). Animal entities can also be subdivided in some classifier systems: by status (e.g. religious significance), relative size, habitat (land, air, or water), and occasionally function (e.g. transportation). This general hierarchy is schematized in Table I.

The actual classification of both animals and non-animate entities very frequently involves properties based on the shape of those entities. The three *basic shapes* seem to be ROUND (i.e. extension in three dimensions, with a spherical contour), LONG (i.e. extension in only one dimension), and FLAT (i.e. extension in two dimensions). Of these three, ROUND and LONG are by far the commonest. (Adams and Conklin (1973) suggested that FLAT might simply be a further elaboration of LONG.) In addition to these basic shapes, many classifiers rely on several secondary parameters as well, although these secondary properties are never used on their own for classification, according to Adams and Conklin. Some examples of secondary properties are listed in (5):

(5) a. rigid versus flexible (sometimes called 'breakability')
    b. relative size
    c. empty versus full (of containers)
    d. regular versus irregular (of shape)
    e. vertical versus horizontal (of LONG entities only)

The different combinations of primary and secondary features of classification may yield large numbers of classifiers and hence of categories (e.g. Berlin 1968).

TABLE I: BASIC CLASSIFICATIONS IN NATURAL LANGUAGES



Different features of shape may be used to classify the same entity at different times as long as that entity possesses the property picked out by the classifier. If the entity is transformed, therefore, as a result of some action, different classifiers may be used for the 'before' and 'after' states. Thus, fresh oranges might be classified as 'round-things', but once peeled and separated into segments, they might become 'parts-of-round-things'.

Some classifiers depend on function, but they often involve properties of shape as well. Adams and Conklin (1973) noted that most function-based classifications seemed to be language-specific and reflect particular cultural orientations. As a result, the entities grouped into a single category might be more disparate than for other classifier categories. In Burmese, for example, elephants and horses are grouped with non-animates as forms of land-transportation.

Classifiers are clearly used to group entities on a principled basis. Occasionally, though, one finds that a group contains an 'odd man out' — an object with no discernible classifier-based relation to other members of the category. For example, Garo puts together *stone, ball, eye, coin,* and (round)*fruit* in the same category because they are all round. But *banana* also occurs with this classifier. This fruit seems to be added in simply because all the other kinds of fruit belong with the 'round' classifier.

To summarize, the classifier systems found both with numerals and with verbs in many different Asian and American languages suggest that much basic categorization depends on the visual form of the entities being classified. Secondary classifications usually involve additional physical characteristics. In addition, some classifications are made on the basis of function, but these tend to be language- or culture-specific, unlike classifications based on shape.

Acquisition of word meanings

As the child begins to acquire language, he is faced by a major problem: that of mapping his knowledge about particular entities onto language. Data on children's early uses of words suggest that at first children may assume that a word picks out only some of the characteristics of an entity (see Clark 1975). For example, many children learn a word like *doggie* or *kitty* very early on. But they then proceed to use this word very differently from the adult. They apply it to horses, cows, sheep, and all sorts of other animals as well as to dogs or cats. Sometimes the criterial property used in the child's over-extension of a word is motion, so the word is used to call attention to all moving objects. Sometimes the criterial property is shape, in this instance usually something like 'four-leggedness', so the word is used to pick out four-legged animals.[1]

[1] From the communicative point of view, the child's use of over-extensions is the result of having a very limited vocabulary. The child's over-extensions, therefore, can be interpreted in two slightly different ways: either he is using a *full over-extension* where the only meaning attached to the word is the one or two features criterial to its use (see Clark 1973a). Or, the child could be using a *partial over-extension* where the critical features for over-extensions seem to be those properties that the child takes as salient in context. In over-extensions, those features form a minimal lexical entry, but they may not exhaust the meaning the child has attached to the word. The use of partial over-extensions would explain why some children

These over-extensions in the child's speech are informative about the kind of meaning attached to his early words. If the child uses the same word for a number of disparate objects (from the adult point of view), we can infer that the meaning of the word for the child includes at least some property or characteristic held in common by those objects. We find many careful accounts of such over-extensions in diaries of children's language acquisition (see Clark 1973a, 1974). Most over-extensions seem to occur between the ages of 1;0 and 2;6 years. (The absence of such reports for older children appears to be highly correlated with the change to asking frequent *what(s) that?* questions and a concomitant rapid increase in vocabulary from about the age of 2;0 or 2;6 on.) The over-extension of particular words may last a few weeks or several months, and the application of each word, of course, is eventually narrowed down by the child's acquisition of new vocabulary. Adults supply new words — an implicit correction of the child's usage — and occasionally correct on truth value (e.g. *That's not a doggie, it's a horse*).

What does the child use as the basis for his over-extensions? First of all, the vast majority of over-extensions appear to be based on the child's perception of the world around him. He uses visual information in many or even most over-extensions, but he also relies on tactile, olfactory, and auditory information too. Over-extensions can be divided according to the criteria children use into several groups: shape, movement, size, texture, sound, and taste. Visual perception appears to play some role in all of these, except possibly for sound and taste. In addition, children produce a few over-extensions that seem to have a functional basis.[2]

The largest class of over-extensions noted in the diary literature consists of over-extensions based on properties of shape. Typical examples of these are shown in Table II. One of the most salient shapes for young children appears to be 'round-&-fairly small', and over-extensions based on roundness are reported extensively. There are also over-extensions based on stick-like properties, on sets of (vertical) stick-like objects, on squares or cube-shapes, and on more complex characteristics of shape such as 'four-leggedness'.

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do not over-extend when their understanding of a word is checked, even though they do when they produce the word themselves (e.g. Moore 1896; Clark 1975).

[2] Some investigators have suggested that the child's first word meanings are based on his knowledge of the functions of objects rather than on his knowledge of their perceptual properties (e.g. Nelson 1973). However, children freely over-extend many of their words in a way that appears to ignore function while relying on perceptual properties (see Bowerman 1975; Clark 1975). In addition, knowledge about some kinds of function appears to be acquired much later on when the child learns about the particular culture he is growing up in (e.g. Andersen 1975).

TABLE II: SOME OVER-EXTENSIONS BASED ON **SHAPE**

Lexical Item	First Referent	Domain of Application*
mooi	moon	cakes, round marks on windows, writing on windows and in books, round shapes in books, tooling on leather book covers, round postmarks, letter 'O'
nénin	breast, food	button on garment, point of bare elbow, eye in portrait, face of person in photo
buti	ball	toy, radish, stone spheres at park-entrance
ticktock	watch	clocks, all clocks and watches, gas-meter, firehose wound on spool, bath-scale with round dial
gumene	coat button	collar stud, door-handle, light-switch, anything small and round
baw	ball	apples, grapes, eggs, squash, bell-clapper anything round
kotibaiz	bars of cot	large toy abacus, toast-rack with parallel bars, picture of building with columned facade
tee	stick	cane, umbrella, ruler, old-fashioned razor, board of wood, all stick-like objects
kutija	cardboard box	matchbox, drawer, bedside table
mum	horse	cow, calf, pig, moose, all four-legged animals

*New objects were 'added' to the domain in the order listed.
(Based on Clark 1973a).

Table III contains examples of over-extensions based on relative size. Children usually seem to pick out fairly small objects, often things or people their own size. Occasionally, a child's use of size as criterial to the use of a word can lead him to split an adult category into two. Schvachkin (1948), for example, reported one child who used the word *va* for dogs. Once he acquired *mu* for cows, he began to include large dogs like wolf-hounds with cows rather than with dogs.

In Table IV, I have listed some examples of over-extensions that seem to be based on texture. In most of them the texture can probably be detected visually as well as tactilely. (It is also clear from these instances that several different criterial features may play a role in the history of the child's mapping of word meanings. For example, the child might begin by focussing on shape and then turn to some property like texture as well or instead (see also Bowerman 1975).)

TABLE III: SOME OVER-EXTENSIONS BASED ON **SIZE**

Lexical Item	First Referent	Domain of Application
babie	baby	cast of 'laughing boy', all persons except adults
fly	fly	specks of dirt, dust, all small insects, child's own toes, crumbs of bread, a toad
em	worm	flies, ants, all small insects, heads of timothy grass
Dina	young girl	all little girls
pin	pin	crumbs, caterpillars
bébé	baby (self)	other babies, all small statues, figures in small pictures and prints

(Based on Clark 1973a).

TABLE IV: SOME OVER-EXTENSIONS BASED ON **TEXTURE**

Lexical Item	First Referent	Domain of Application
sizo	scissors	all metal objects
bow wow	dog	toy dog, fur-piece with animal head, other fur-pieces without heads
wau-wau	dogs	other animals, toy dog, soft house-slippers, picture of old man dressed in furs
p'asaka	powder	dust, ashes
puppe	doll	toy rabbit, all other playthings (except toy bell — only non-cuddly object)

(Based on Clark 1973a).

Lastly, another fairly large class of over-extensions consists of those based on movement. Motion, of course, is another property of objects that is most easily detected visually. Examples of these over-extensions are shown in Table V.

In addition to the kinds of over-extensions listed in Tables II-V, children also produce some over-extensions that might be called functional over-extensions. They nearly all seem to involve some change of state resulting from an action. Some examples are listed in Table VI.

One notable absentee from the kinds of over-extension found in chil-

TABLE V: SOME OVER-EXTENSIONS BASED ON **MOVEMENT**

Lexical Item	First Referent	Domain of Application
titi	animals	pictures of animals, all things that move
sch	sound of train	all moving machines
bird	sparrows	cows, dogs, cats, any animals that move
tutu	train	engine, moving train, journey
ass	toy goat with rough hide, on wheels	things that move (e.g. animals, sister, wagon), all things that move, all things with a rough surface

(Based on Clark 1973a).

dren's speech is color. Studies of children's preferences for matching a standard object where shape, size, or color are possibilities also show that young children tend to choose shape first, then size, and only rarely color (see Clark 1974).

TABLE VI: SOME FUNCTIONAL OVER-EXTENSIONS

Lexical Item	First Referent	Domain of Application
aga (allgone)	said when had drunk all milk	said of anything put out of sight, disappearances of all kinds
our (ouvrir)	said of father's door	said of piece of fruit peel, of a box, of a peapod, of his shoes he wanted unlaced
atta	departures	opening or closing of doors, raising box lid, any disappearance of object from sight

(Based on Clark 1973a).

Some comparisons

One basic distinction found in classifier systems is that between animate and non-animate. At first glance, there does not appear to be anything directly analogous in children's speech. However, consider for a moment what indicates animacy. One salient characteristic is that animate entities can generally move of their own volition. And, as we see from Table V, children often make over-extensions based on movement. From the time children combine words to form two word utterances, we find additional syntactic and semantic evidence that the distinction between animate and non-animate is a fairly basic one for the child. For example, Brown and Fraser (1964) found that adjectives like *tired* occurred only with nouns that had animate referents. Bowerman (1973) found that only [+animate] nouns occurred in initial position in two word strings, and [−animate] in final position for the Finnish children she studied. For one child, even, nouns referring to vehicles had the same privileges of occurrence as nouns denoting animate entities (see also Braine 1976). If movement is indeed one of the more salient characteristics of animate entities, then it is not surprising that children should pick on it when they begin to work out what words mean.

The most frequent basis for over-extensions, however, appears to be shape. Shape is also widely relied on in classifier systems where the properties of being ROUND, LONG, or FLAT seem to provide a primary basis for categorization. Some of the examples cited by Adams and Conklin (1973) are listed in (6)-(8):

(6) a. *Indonesian:* fruit, pea, eye, ball, stone [round]
  b. *Laotian:* sun, moon, star, plate, pot, eye, stone [round]
  c. *Thai:* bead, stone, seed [round-&-small]
    globe, fruit [round-&-somewhat-larger]
    stone, lump of earth, irregular lumps [round-&-irregular]

(7) a. *Nung:* tree, bamboo, thread, nail, candle [long]
  b. *Chrau:* stick, fish, pencil, knife, snake [long-&-thin]
  c. *Trukese:* tree, canoe, stick, pencil, cigarette [long-&-thin]
  d. *Assamese:* rope, thread [long-&-flexible]
    wood strip, tablet, strip of bamboo [long-&-flat]
    bamboo, piece of wood, spear [long-&-round]

(8) a. *Chrau:* mat, cloth, small gong, clothing, turtle [flat]
  b. *Kachari:* leaf, fan, piece of cloth [flat]

The lists of category members grouped by the same classifier bear a strong resemblance to the lists of objects included in children's over-extensions. For example, compare the following collections of round things and long things grouped together by young children with those in (6) and (7):

(9) a. *English:* watches, clocks, gas-meter, firehose wound on a spool, bathroom scale with a round dial [round]
  b. *French:* nipple, point of bare elbow, eye in portrait, face in photograph [round-&-small]

c. *Serbian:* coat button, collar stud, door-handle, light-switch, anything small and round [round-&-small]

(10) a. *English:* stick, cane, umbrella, ruler, old-fashioned razor, board of wood [long]

b. *English:* bars of cot, abacus, columned facade of a building [long-&-vertical-&-parallel]

There do not appear to be any examples of flat objects being grouped together as the result of an over-extension. This may be because FLAT is really a special case of LONG, as Adams and Conklin (1973) suggested. In the case of ROUND and LONG, however, the children's data suggest that these properties play an important role in the formation of linguistic categories (Table II).

Children also use much more complex features of shape when, for example, they over-extend a word to all four-legged animals. The characteristic of being four-legged is also used as the basis for some classes of animates in classifier languages. Animal classifications may also be split on the basis of size. For example, the classifiers used in South East Bougainville divide animals into large, small, and aquatic. Relative size (usually small relative size) is also fairly common in children's over-extensions (Table III). For children, though, size tends to be less important than shape though it may be preferred over color in some matching tasks (Clark 1974).

Color alone never seems to be used as a basis for over-extension. Interestingly, Adams and Conklin (1973) found no classifiers in the languages they surveyed that ever used color as a basis for categorization. Here again there is a strong parallel between the child's first attempts to map his knowledge onto language and the categorizations made by classifier systems.[3]

The last group of over-extensions I will consider are those based on texture (Table IV). The child is again relying on a perceptible characteristic of an object in using a word. He can identify texture visually or tactilely. The distinctions within classifier systems that seem to be closest to this type of over-extension tend to be secondary properties. For example, the flexibility of an object may make a difference to its surface appearance, and hence to its texture. Berlin (1968) reported that Tzeltal speakers use three classifiers for small, medium, and large *soft, wool-like* spheres. What is criterial is the texture. Table IV shows that where children use texture, their over-extensions often involve soft or furry materials.

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[3] I am indebted to Joseph Greenberg for the observation that color rarely if ever appears in dictionary definitions, compared to the use of shape and substance. Color appears to play a minimal role in the lexicon of most languages.

Visual perception plays an important role both in children's over-extensions and in the use of many classifiers. In both cases, objects are categorized primarily on the basis of shape, and the same properties of shape appear to be relevant in acquisition and in classifier systems. Roundness and length (both vertical and horizontal extension) appear to be very salient.[4] Children appear to pay less attention to flatness *per se* in their over-extensions, but horizontal surfaces are important in other domains (see Clark 1973b). Secondary properties used for classification in combination with ROUND and LONG also crop up in children's over-extensions. In addition, children seem to draw a distinction between animates and non-animates in their language at a very early point in acquisition. Some of the evidence for this comes from over-extensions based on movement, and some from the co-occurrence constraints on noun-adjective sequences and positional preferences in two word utterances.[5]

Many of the categorizations made by classifiers and many of the child's over-extensions depend on various features of shape. I want to propose that both classifier systems and children's over-extensions reflect a basic categorization process that goes on *first* at the non-linguistic level. Both classifiers and over-extensions provide information about the kinds of organizational principles people rely on when they try to make sense of the world around them. One way that people seem to organize entities is to group them on the basis of their perceptible properties, with shape playing a very important role (see Rosch 1975, 1976). The data from children suggest that some properties of shape may be more salient than others and thus more likely to be used in categorization.[6] The same properties that appear to be salient for children also appear in classifier systems as some of the primary dimensions for categorization. This suggests that ontogenetically classifier systems may have evolved the way they did because they reflect precisely those distinctions that the child draws on when he learns to map his knowledge onto language. Within classifier systems, then, one might expect to see a progression from systems that only distinguish animates from inanimates, to systems with more and more complex subdivisions using several dimensions at once to produce a large number of classifier-categories. For example, a language like Tenejapa Tzeltal contains over 500 classifiers (see Berlin 1968, Appendix II).[7] At the same time, classifiers in general are redundant. They group together categories that have already been picked out by the words used to label them in the language. In the case of the child, the problem is rather different: the child is working on what it is that a label picks out in the first place when he uses it to talk about a particular entity. In both cases, the particular categorizations appear to have a cognitive basis when we look at the meanings of different classifiers and at the child's early over-extensions.

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[4] Children's attention to long objects, particularly those with a vertical orientation, is widely documented in both observational and experimental studies. Verticality seems to be particularly salient for young children (see Clark 1974, and references therein).

[5] Another example of noun-adjective constraints comes from Piaget (1951) who noted that children first predicated *alive* of objects that moved or had some effect on a prior state, e.g. bicycles, people, the sun, the wind, candles (the latter possibly because they give light, possibly because of the movement in the flame). Later, *alive* is restricted to just those objects capable of spontaneous movement (people, animals, the sun, etc.). And then finally, the application of the adjective is narrowed down to animals, people, and plants. This kind of example also gives some support to the suggestion that the distinction equivalent to [+ animate] first made by the child is actually based on the child's perception of movement as a characteristic of the class.

[6] Young children's spontaneous sorting and grouping of blocks of different shapes, sizes, and colors, suggests that shape is in fact a primary factor in categorization (e.g. Ricciuti 1965).

In summary, both classifier systems and the child's over-extensions appear to depend on an *a priori* categorization process in which similarities of shape and other perceptible properties play a central role. At the same time, the distinctions made by using classifiers and those the child appears to make suggest that some properties of objects are more salient conceptually than others. This would account for the prominent role certain distinctions play in classifier systems as well as in over-extensions. Both classifiers and over-extensions, therefore, may be able to cast some light on the cognitive capacities we use in the formation of natural categories.

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[7] In numeral classifier languages, classifiers are only used when a numeral quantifies the noun, so textual occurrence may be fairly rare. Studies like Berlin's, therefore, are essential for providing an accurate picture of the precise distinctions made by classifiers.

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The Devil's Case: PIE as Type II

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1. Introduction[1]

The case that Proto-Indo-European (PIE) had a subject-object-verb (SOV) order was originally stated by Delbrück with reference to Sanskrit prose (e.g., 1878), and then extended to Vedic (e.g., 1888), and then to PIE (e.g., 1897). Since then it has been argued and elaborated variously by leading Indo-Europeanists such as Watkins (1963), Dressler (1971), and most conspicuously by Lehmann (e.g., 1974) and some of his students and followers. One consequence of this widespread position has been to obscure the case for alternative hypotheses. The well-wishing non-Indo-Europeanist now generally assumes that what is still in fact only *one of three hypotheses* is supposedly a tested theory, that PIE was not only SOV but a fairly harmonic system of Greenberg's type III, with adjective-noun order (AN), genitive noun order (GN), standard-adjective order in comparison (SA), the postposing of 'prepositions' in adnominal position, and the preposing of relative clauses; in terms of the 'topic-comment' model, all these units are in the modifier-modified order. A dogma and a rhetoric have been established, particularly by Delbrück and Lehmann, to which the statement that follows may serve as a salutary antidote.

In an earlier publication (1975) I reviewed many theoretical issues,

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[1] Earlier versions of this paper were read to the Linguistic Soceity of America (San Diego, 1973), and to the Chicago Linguistic Society (1974). Of the persons who helpfully criticized earlier versions of the monograph (as gratefully acknowledged therein), I would particularly like to reiterate the names of John Attinasi, Deborah Gordon Friedrich, Henry Hoenigswald, Gary Miller, and James Redfield. An earlier version of the present article received helpful comments from Marshall Durbin, Miriam Klaiman, and Ernst Pulgram. I would also like to repeat my gratitude for their statistical-textual work to Bonnie Urciuoli (Latin), Bruce Lincoln (Avestan), and Barbara Knowles (Hittite, and the Vedic counts on comparison and the locative auxiliaries). Doug Adams and Jay Jasanoff provided much needed assistance with Tocharian. For fuller details and bibliography the reader is referred to Friedrich (1975).

earlier interpretations, and facts, including a considerable amount of heretofore neglected patterning (e.g., in Homeric Greek, Italic, Old Armenian, Avestan, and Tocharian). Such a relatively balanced view seemed necessary, indeed urgent, at the time because of the persistent neglect of basic evidence, particularly from Celtic, Old Armenian, and Homeric Greek, and because of the equally persistent, nineteenth-century Sanskritophilia, and the Sturtevant-derived over-emphasis on Hittite. One outcome of my study was that AN and SV orders could be posited with confidence for PIE, although this too was a matter of probability. Otherwise, a considerable degree of indeterminacy marked both the empirical case for the other orders (e.g., the OV/VO crux), and even the general chances for reconstructing a proto-syntax — at least in this key instance, in contrast with the chances of reconstructing phonology and morphology.

I would still today adhere to such a mode of indeterminacy in syntactic reconstruction, and I still think that a polemical case for PIE as type II (SVO, etc.) does *not* follow logically from a balanced review of the evidence — partly because of the serious arguments that remain in favor of both type I and type III: I can see, on the other hand, that my earlier attempt to be balanced and detailed, and to entertain diverse alternatives and theories, may have masked the point of greatest interest to fellow scholars: the cumulative power of the combined arguments for the specific hypothesis that PIE was type II. My present objective is to pull together these scattered arguments. Such a synthesis will, inter alia, illustrate with relative clarity the crucial role of typology in reconstruction, including syntactic reconstruction; I will knit the case as tightly as possible to the results of Greenberg's typological studies. This integrated case for PIE as type II can in the future be dialectically counter-posed to the traditional Delbrück-Lehmann case for type III, and also to what may be the 'angel's case' for PIE as type I (e.g., Hall and Hall 1971; Miller 1975). The present paper, while at one level a polemic in favor of type II, purports to be, at a deeper and more significant one, a strengthening of the ultimate and general case for indeterminacy: it will always remain impossible to construct a case that renders trivial the main alternative hypotheses. On this admittedly somewhat ironic note let us now turn to a polemical synthesis of the arguments for PIE as type II.[2]

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[2] My general theoretical position has many affinities with that of Chao (1934). Greenberg's system clearly has certain limitations, but I think we should reconstruct within a framework of explicit typology (rather than just referring to it), and agree with many of his points about probability and implication. *Frequencies* of alternate patterns, based on an adequate sample, are an invaluable supplement to either/or, categorial rules, and sometimes are preferable — particularly when, as is often the case, the categorial rules turn out to be rules of frequency, or the over-generalizations of an established authority.

2. Syntactic arguments

2.1. *Adjective order.* Most Indo-European stocks have as their dominant order the noun preceded by the adjective — I am speaking mainly of attributive adjectives — and this same order is also basic in compounds (Meillet 1964:290). But note in the first place that Proto-Slavic and Proto-Celtic both exemplify the opposite, NA order, and that in the latter of these stocks the rule is essentially exceptionless. Second, granted that AN is *theoretically* harmonic with OV and other modifier-modified orders, the IE stocks do vary enormously in the degree to which the rule is obligatory. This ranges from Homeric Greek, Old Latin, and Gathic Avestan, where it is weakly dominant in terms of diverse criteria, to Vedic, where it is fairly regular, to Hittite, where it is rigidly followed.[3] Third of all, while AN is harmonic with OV in terms of the topic-comment model, it is also true that the empirical, statistical correlation of these two variables is egregiously inconclusive; one table in Greenberg, for example, shows eight NA and five AN rules in 13 VO systems, and five NA and six AN rules in 11 OV systems (1966:85). Speaking more generally, the *disharmonic* combination of AN and VO is widespread and indeed characteristic and highly stable in the European area, as in Romance (except Latin), the Slavic and Germanic languages, and also Albanian. The combination may be an areal sprachbund phenomenon (it occurs, also areally, in a number of Chinese languages). Yet more generally, "most exceptions to harmonics involve adjectival order" (Masica 1972:35). These cross-linguistic, typological generalizations are corroborated by deeper qualitative and developmental considerations. For example, the order in Mycenaean Greek was actually NA by a weak rule (Panagl 1973), while in Homeric Greek, as noted, it was AN, also by a weak rule. By Attic times we get a strong AN rule, which has become essentially obligatory in Modern Greek. Over the same period the order of verb and object has shifted toward ever greater obligatoriness of VO. In other words, the drift has been simultaneously toward two disharmonic patterns. The evidence for AN in Homeric Greek or PIE cannot, then, be said to support the type III hypothesis.

2.2. *Genitive order: NG.* GN is dominant in Tocharian, Iranian, Indic, and Hittite, but the rule is powerful only among the latter two of these four eastern stocks. In Hittite the relative frequencies of GN over NG run about five to one (and the exceptions involve predicate genitives, Akkadian factors, and so forth). The GN rule is somewhat weaker in Vedic, although by the time of Sanskrit prose the noun is only preposed for emphasis, or in incomplete sentences. In Gathic Avestan one finds that GN is somewhat more frequent, but only by a ratio of three to two. By the time of Younger Avestan NG is definitely favored. Old Persian is generally GN, but NG occurs under three specific conditions (e.g., when N and G are linked by *hya*). Tocharian, finally, usually prefers derived possessive adjectives to genitives, an archaic pattern that also occurs with various degrees of power in Baltic, Germanic, Italic, Old Armenian, and Greek, and most particularly in Slavic. Slavic contacts may in fact be reflected in the Tocharian pattern (Lane 1970), or both may be inherited from PIE (Ivanov 1959:35).

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[3] Delbrück's early spade-work on Sanskrit prose (1878) deeply influenced his later generalizations about Vedic (e.g., 1888), and both of these, in turn, his generalizations about 'Indo-Germanic'. These successive strata and their interrelations clearly call for a specialized treatment by a Sanskritist-cum-syntactician — particularly in light of the deep influence of Delbrück on Lehmann, and others. As Avery pointed out in 1881, speaking of Delbrück, "It would appear that the author's views rested upon general impressions from reading, rather than upon an enumeration of instances".

Not only do the majority of stocks have NG order as dominant, but their number includes such archaic systems as Proto-Slavic, Proto-Germanic, Homeric Greek, and also Old Armenian; in the latter, descriptive genitives are often used instead of adjectives, and are always in NG order (Tumanjan 1971:188-9). Within Italic, even the Old Latin of the Twelve Tables and the chronologically early and colloquial Plautus are both NG (as is the less colloquial Caesar). In Old Irish the rule is NG, except for relative pronouns. Thus, while the reconstruction for PIE must remain inconclusive, the case is somewhat better for PIE as NG, which is of course harmonic with VO.[4]

2.3. *Comparison: AS.* AS order is found in the majority of stocks, including Armenian, Celtic, and in Old Norse, as possibly other early Germanic languages. The normal Latin order seems to have been Pivot + Adjective + Marker + Standard.[5] In Greenberg's terms (1966:88), such AMS systems are mainly associated with type II, sometimes with type I, but never with type III; AS is harmonic with VO. The patterns remain unclear for Old Iranian. SA is definite only in Tocharian, Hittite, and Sanskrit, but in Vedic the frequency rule is not a strong one (11 SPA and 15 PSA by one recent count). Delbrück's classic on early prose does not actually deal with comparison.

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[4] Relatively intensive analysis of Homeric Greek has uncovered a subcategorial breakdown, with NG for 'syntactic genitives' such as such as the subjective genitive, GN for proper names and 'genealogical genitives', and NG and GN about equally elsewhere. Other stocks also show such differences depending on the type of genitive (e.g. Slavic, Old Persian). One hopes that typology and reconstruction will be pursued in terms of such finer subcategorizations rather than the relatively gross taxon of 'genitive'. NG is claimed to the usual order in Old Lithuanian by Ford (1969:166), but I was informed by Z. Gołab (as this went to press) that GN is actually dominant.

[5] Pivot and Standard are partly confused in Friedrich's (1975) section on comparison in Italic, and hence are related inconclusively to the argument — as contrasted with the above, corrected inferences.

A basic source of confusion has been the focus on the two variables of adjective and standard (which reflects, in turn, the undue influence of the model of Turkish and Japanese, and other rigid SOV languages). If we include the variables of Pivot and Marker, as would Greenberg, we find that Homeric Greek, for example, has the pivot initially, whether as PSA or PAS, by a ratio of over three to one; this initial position of the pivot is harmonic with the subject-initial formula. The roughly equal frequency of SA and AS, on the other hand, implies an SVO order because the latter, since it includes both modifier-modified (SV) and modified-modifier (VO) orders, which are disharmonic with each other, is a more flexible triad and more compatible with otherwise conflicting patterns. What we obviously need is a full-scale comparative study of comparison in all the stocks, taking Pivot and Marker fully into account and relating the results to what is known typologically and in terms of the logic of comparison (Puhvel, 1973).

Two PIE "comparative morphemes" (Kuryłowicz 1964) can be reconstructed, the appositive **-tero-* and the elative **-yes-*, and each is backed by large sets of excellent correspondences in six major stocks, sets that have long been accepted by Indo-Europeanists.[6]

In sum, it seems more probable that AS was the order in PIE comparison, given the scope of evidence in its favor, and the weakness of the evidence for SA. This is further support for the type II hypothesis.

2.4. *Relative clauses: MCRC.* The preposing of relative clauses, so diagnostic of rigid type III languages, is, among the early dialects, limited to Hittite and Sanskrit, and even here has to be seriously qualified. A quarter of the Hittite 'definite' clauses (with definite antecedent) are postposed (Held 1957 — granted considerable problems of the meaning of 'definiteness' and with whether or not something is definite). There is also much postposing of relative clauses in the Rig Veda and other early Sanskrit poetry; in "very nearly 4,000" passages in the Rig Veda, relative clauses actually followed by an extremely slender majority; specifically,

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I am citing from Old Norse in particular because of the accident of having adequate analysis by Deborah Gordon Friedrich, based on hundreds of examples and a careful check of Heusler and other authorities. But in Old English, another early Germanic dialect, we also find a subtype of II. Nor is Gothic verb-final. I have not gotten to J. Smith's doctoral thesis, but remain puzzled by the widespread claim that early Germanic dialects provide some of the best evidence for the rigid SOV hypothesis.

[6] The two comparative morphemes, like the two relative and the two reflexive pronouns, raise the question of a constructive relation between etymology and syntactic reconstruction; powerful etymologies of key syntactic particles cannot be dismissed because of deductions from a syntactic hypothesis.

the relative clauses followed by 50.8%, and preceded by 46.8%, and otherwise stood within (the "relative word" stood at the head of its clause 65% of the time; Avery 1881). The initial conclusion should be that the Vedic evidence does not support *either* hypothesis, and will not until we get more sophisticated analyses in terms of other-than-statistical criteria for markedness.

Postposing is either more frequent or is simply obligatory — and this even in the eastern IE dialects of Tocharian[7] and Old Iranian, as well as in Classical Latin, Old Armenian, Old Irish, and Homeric Greek. Many of these languages, notably Homeric Greek, freely concatenate postposed strings of relative clauses (with, in some cases, only the first one of them introduced by a relative pronoun). In Old Persian the antecedent head was often incorporated into the postposed relative clause (Kent 1950:84). Relative clauses are *always* postposed in Old Norse prose, and comparison of the early Germanic dialects indicates that postposing should be posited for Proto-Germanic. The general postposing of relative clauses is harmonic with the preposing of locative auxiliaries in adnominal position (on which more below), and constitutes strong evidence for the type II system.

Two forms, **yo-* and **k^{w}e-*, have been reconstructed on the basis of sets of excellent cognates that have been accepted by Indo-Europeanists for decades, and both of these forms functioned to some extent as relative pronouns (e.g., Gonda 1954 on **yo-*). For example, both of these protoforms have reflexes with relative pronominal functions, or the closely related clause-linking functions, in Greek, Hittite and Celtic. Almost all languages have semantically differentiated relative clauses which, at least in VO languages, are introduced by relative pronouns that are usually functionally specialized to some degree. The powerful and well-established evidence for a relative pronoun (or pronouns) in PIE argues for a VO order.

2.5. *Preposition-adverbs or 'locative auxiliaries': the case for preposing.* In ancient dialects such as Hittite or Homeric Greek the units in question (1) can stand in immediate construction with a noun, with which they share a dominating noun phrase node, in which case they function to disambiguate case subcategories; or (2) can stand in immediate construction with a verb, with which they share a dominating verb phrase node, in which case they function to disambiguate aspectual subcategories; or (3) can modify about equally both a noun (phrase) and a verb (phrase), with both of which they share a dominating sentence node; or (4) they can stand as independent overt predicators. Both the case and the aspect categories are spatio-temporal (compare Wackernagel 1924:210; Dressler 1971). And they are syntactically auxiliaries. They constitute a grammatical subsystem in their own right with distinctive semantic and syntactic functions.[8] I am calling them 'locative auxiliaries' — a term that Ivanov coined independently somewhat earlier (1973:60). This nomenclature seems preferable to calling them (and dealing with them as) 'preverbs' in one part of the grammar, and as 'adverbs' in another, and as 'prepositions' or 'postpositions' in yet another part.

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[7] To be more specific, a definite relative clause usually follows the head in Tocharian, whereas an indefinite precedes — a patter that is shared in various degrees by Sanskrit, Greek, Hittite, and Old Irish (and Eddic poetry).

The locative auxiliaries are of differential relevance to the problem at issue here. The evidence is that they normally preceded the verb, as is consonant with the type II hypothesis. But they can also be expected to precede the verb in a type III language such as Hittite, where the strong verb final rule over-rides the rule for modifier-modified order.

The key, moot question involves adnominal position: were they preposed or postposed? And this is especially pertinent since what remains a hypothesis about the locative auxiliaries has been made into an anchor for the type III hypothesis (Lehmann 1972:983-5).

Locative auxiliaries are postposed adnominally in Hittite (J. Friedrich 1960:179), but this is most probably something that evolved independently within Anatolian, and specifically in Luvian (Ivanov 1963:181-82). Postpositions do dominate in Tocharian, being twelve in number, but many could be preposed, and three locative auxiliaries were primarily prepositions, usually occurring with the oblique case (Krause and Thomas 1960:171).

About Vedic Sanskrit, Macdonnell (1971:208) says, "As a rule these [adverbial] prepositions follow, but also often precede, their case." Actually, postposing is about four times as frequent as preposing. For Classical Sanskrit prose, Delbrück's impressionistic and highly normative statements about rigid postposing have to be compared with the fact that the rule is a statistical one of somewhat greater power than in Vedic. As for the norm, the authoritative Renou says, "The place of the preposition is free . . . in that, in principle, it can appear before or after the noun" (1968:169-70). This leaves us with a long-term drift from a *frequency* rule for postposing in Vedic, to a *higher* frequency in Classical (but not an obligatory rule at all), to the strong (albeit *not* rigid) postposing of the modern Indic languages. The long-term diachrony points toward a greater degree of *preposing* as we go back toward PIE.

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[8] Bolinger (1971) has coined the apt term 'adprep' (to capture the combined prepositional and adverbial functions in English), and discovered a number of interesting properties. See Brunel (1939) for a superb analysis of the aspectual functions of locative auxiliaries in Attic Greek.

In all the remaining stocks preposing is a high frequency rule, or obligatory, and this includes even Old Iranian and Old Armenian. The few postpositions in Greek are not necessarily vestiges of an original PIE pattern, but could reflect a partial adjustment to a substratum. Celtic is, of course, exceptionlessly preposing.

In sum, PIE was probably ambivalent, with preposing somewhat more frequent and less marked. The standard dogma of postpositions in PIE is strongly supported by only one stock: Anatolian.

The conclusion that the PIE locative auxiliaries were probably preposed has substantial consequences for an overall theory. On the one hand, prepositions and postpositions are the *dependent* variables in Greenberg's system (e.g., if SOV, then postpositions), and I think that for the present the value of the implications has to be taken in these terms. By the same token it is not valid to simply deduce SOV from postpositions — even if the case for the latter were airtight. On the other hand, I think that the probable preposing of the locative auxiliaries has to be evaluated within a cybernetic framework: the probability of such preposing significantly decreases the likelihood that SOV was dominant, and increases the likelihood that PIE was VO, whether I or II.

2.6. *The position of the verb.* The relatively good case for a verb final rule, with evidence from most of the most valued stocks, has without doubt contributed to the view that PIE was necessarily SOV, and hence type III. The stocks that are more or less verb final, at least in their early stages, include Anatolian, Indic, Iranian, Tocharian, and in the west, Italic and Baltic (e.g., Lithuanian). But the Anatolian (i.e., Hittite) rule is not by any means rigid. The rule is actually weak in Vedic, and even for Classical prose the patterns are problematical; Staal (1967) tried to show that object/verb order was indeterminate, and adduced some good arguments, although most experts in Sanskrit would agree that the basic order was OV (e.g., Gonda 1959). OV order certainly did become regular, albeit not rigid, in the modern Indic languages such as Hindi. Thus the drift was from *less to more* rigid OV ordering — certainly not a good case for OV in pre-Indic or PIE!

Old Iranian was far from being strongly OV. Even in Avestan the relative frequencies, by one recent count of an adequate sample, were SOV 29, SVO 21, OSV 14, and OVS 13; thus, while OV outnumbers VO by 56 to 21, SOV outnumbers SVO by only 29 to 21. In Old Persian, SOV is definitely dominant, but has to be qualified to the degree that non-direct object complements often *follow* the verb, as do second and subsequent objects (Meillet and Benveniste 1936:239-40).

Let us turn to the other SOV stocks. The verb was normally final in Old Lithuanian (Ford 1969:165), and probably was in early Baltic (to judge from the evidence in Berneker 1900). Old Latin and the earlier Classical authors were SOV — granted some exceptions such as the first part of *The Gaulic Wars*, and granted that there are many singular, disharmonic combinations with modified-modifier dyads so that a pure type III never obtains in Latin. By late Classical times over a third of the clauses were in SVO order. The Tocharian rule, finally, is a high probability one that is specifically for prose (Krause 1959:83-84), and all the verbs in some prose texts are final. In sum, the case for SOV from the SOV languages themselves has to be qualified in diverse ways.

The majority of Indo-European stocks are VO, and this includes the basically SVO systems of Albanian and Proto-Germanic; Old English, for example, is SVO, with both VSO and SOV as frequent alternates, and Old Norse, to take a second, was decidedly SVO, with VSO as the main emphatic, marked order (e.g., in conditional clauses without a conjunction). Old Armenian is SVO but shows VSO as the main alternate — almost of equal frequency. Common Slavic was basically VSO (if we depend mainly on Old Russian and Old Serbian, as Berneker did). More recently, SVO has become the most widespread order in the Slavic languages, except for the strong SOV patterns of West Slavic. The dominance of verb-initial clauses in Old Russian admittedly depends in part on the conditioning environment of initial links, and adverbs of time and place. But these clauses can not be 'discounted', given the normalcy of paratactic clauses in syndeton in the early Indo-European dialects in general.

Celtic, last of all, is consistently VSO. Archaic Old Irish has alternatives with verb final under tmesis and Bergin's Law (the latter without a 'preverb'; Dillon 1947; Watkins 1963). I have argued (1975) that these are apt to be, not relics of an ancestral rule and, ultimately, reflexes of SOV in PIE, but rather, marked alternates within a particular, sophisticated literary tradition; they occur only in poetry, rhythmical prose and the rhetoric of the saga texts; as Binchey has said (1966), the examples are drawn from a "highly formalized (and perhaps deliberately obscure) style".[9]

In Homeric Greek SVO and SOV are of about equal frequency, and OV is more frequent by about three to two — but these generalizations mainly reflect the tendency not to have the verb in initial position, and the almost exceptionless rule against VSO. On the other hand, a number of qualitative, structural factors suggest that SVO was the less marked order. One such factor is 'gapping' (the deletion of coreferential verbs in conjoined clauses with subjects and objects that are not coreferential). Gapping is rare in Homeric Greek, and then usually to the right, as would be consonant with an SVO language (Ross 1970). The rarity of *any* sort of gapping would argue against an underlying OV structure (Maling 1972:105-6). Moreover, the long-range drift in Greek is from weakly SVO in Homer to stylistically motivated SVO and SOV in Attic Greek writers (Dover 1960), to increasingly regular SVO as we move into Modern Greek — no better a case for OV than Indic provides!

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[9] Other points can only be mentioned here. The archaic 'conjunct endings' which figure centrally in Watkins' statement, also occur in non-final position. Wagner (1967:222) has claimed that all the examples for 'Bergin's Law' are based on conjecture, and that clear and unambiguous examples cannot be found in "straight-forward Old Irish prose". He also claims that "practically all" the examples of tmesis are irregular constructions with metrically bound alliteration. Finally, "neither the remains of Continental Celtic or Middle Welsh verse warrant or support a theory that the final position of the verb is original in Celtic" (*ibid.*).

Further evidence for VO is that the inflected auxiliary normally precedes the main verb in Homeric Greek and the other ancient dialects. Greenberg actually states the matter in terms of VSO versus SOV, as follows (universal #16): "In languages with dominant order VSO, an inflected auxiliary always precedes the main verb. In languages with dominant SOV, the inflected auxiliary always follows the main verb." This alone would be a strong argument against an underlying or earlier SOV in PIE, but Greenberg's chart (1966:84) shows that in SVO languages the order AuxV is more frequent than VAux by *seven to one*. Universal #12 states that interrogative particles will come initially in VO languages, but that this is not the case in OV languages. These patterns are connected with wider facts about PIE auxiliaries of various kinds: the negative **mee*, the interrogative **ne*, the augment **e* (a preterite auxiliary probably derived from a clause connector). The fact that preverbal auxiliaries were used for these meanings argues for VO order and against OV order in PIE.

A major pattern in the early dialects is the VO or 'Menelaus' type of compound, although limited in distribution to Homeric Greek, Germanic and Indic (Hirt 1928:34). While the evidence for OV order is also good (see Salus 1965), these VO orders may be more archaic and reflect the PIE pattern (Watkins 1969:#67; Meillet 1964:290). The fact that VO compounds often involve epithets may be connected with their extreme archaicity.

Last, there is excellent evidence from Sanskrit and elsewhere that the verb in main clauses came in the second, enclitic position, and hence before objects and other complements. SV(O)) order is strongly implied by one of the relatively sure inferences in PIE syntax: Wackernagel's Law (1955:95, 187-89).

Taken all together, the evidence for VO order, whether part of SVO or VSO, seems appreciably better than that for SOV.[10]

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[10] The verb (e.g., as morphologically defined) is still the most powerful and independent unit variable in syntactic typology. The hierarchy of PIE sentence types would begin with V

3. Areal arguments

As already implied above, some of the most powerful arguments for PIE as type II are areal.

The type II stocks are all geographically central, notably Germanic, Greek, Albanian, and Armenian; Slavic gives excellent evidence for type II. They are in or near the most likely 'homeland' area of 'South Russia' and Hungary, abutting on the Balkans and the Caucasus. Armenian has been influenced by the Caucasian languages, as in the use of the genitive case for the agent-subject. On the other hand, the relatively northern Baltic stock is generally type III, most probably as a result of interaction with Finno-Ugric peoples. The fact that central stocks are closest to the posited PIE syntax is consonant with recent findings in lexicology (e.g., Friedrich 1970), and morphophonology (Gamkrelidze and Ivanov 1975).

The four stocks which provide most of the evidence for type III, and, in particular, for SOV, are *all eastern* — and the distant east in the case of Indic, and Tocharian (Chinese Turkestan). And yet the second of these far eastern systems still preserves type II vestiges after over two thousand years of influence from type III languages, particularly of Finno-Ugric (Krause 1951:196), and Ural-Altaic. Indic, similarly, went through a well-attested and well studied process of 'Indianization' whereby the phonological, morphological, and syntactic systems were drastically restructured as a result of intense, long-term interaction with Dravidian — which is of course a type III of the rigid sort (Bloch 1934; Emeneau 1956). A typical example of such syntactic restructuring in the direction of SOV is strings of non-finite verb forms closed off by a finite verb form. Especially convincing here is the fact that the intrusive Munda languages were similarly restructured because of contact with Dravidian (Pinnow 1960).[11]

Within Old Iranian, Old Persian stands between II and III. Gathic Avestan is only weakly type III, and this degree of modifier-modified order could well reflect contact with Altaic speakers during the first and second millennia B.C.[12] Younger Avestan (circa 350 B.C.) has already

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(with an S necessarily understood), with a subtype (S)V_{intrans}; this would be followed by (S)V_{trans}O, with VSO and SOV as frequent marked alternates, and so forth. Other excellent arguments for (VO) (e.g., SVO) include the very archaic *prepositive* conjunctions such as *nu*, *so*, and *to*, and the major pattern whereby all but the rightmost of a conjunction is deleted (Miller 1975:42-3).

One of Greenberg's complex implicational rules works for PIE and a number of stocks such as Greek: if SVO and GN and Prep, then AN.

[11] Similarly, North Germanic shows the influence of Finno-Lappic in the suffixation of the definite article, the reflexive, and so forth. Both Spanish and French dialects near the Basque area show the effects of Basque, Armenian the effects of Caucasian, and so forth.

[12] Dravidian and Altaic were formerly contiguous, and are close typologically. A genetic connection, first posited and argued by Caldwell (1956), has recently been revived and expanded by Tyler, with massive lexical evidence (1968).

reverted to a type II. The relatively strong verb final rule throughout Iranian may well be an areal phenomenon; Akkadian actually combines Semitic type I features with such a verb final rule. One may assume a syntactic sprachbund, with considerable interaction between the literati and aristoi of these first millenium B.C. civilizations (e.g., the Achaemenid court in western Iran was of course relatively close to Babylon); on the other hand, the influence of Greek and Aramaic must have been considerable during these centuries (Bowerman 1970). In any case, the same combination of verb final and type II orders continues through to the present day in Persian, and Iraqui Arabic, and other, adjacent languages (i.e., still including both Indo-European and non-Indo-European languages).

The speakers of early Anatolian probably spent many centuries moving through the Caucasus before settling in what was to become their imperial center in central Anatolia. As Gamkrelidze says, "the penetration of Indo-Europeans into Asia Minor did not have the character of a sudden invasion . . . it must have come about apparently as a long process of infiltration" (1970:142). The languages most relevant to this long process of interaction are those of the northwest Caucasus, and these are highly conservative languages of type III. Typologically, Hittite syntax is closer to Ural-Altaic and the northwest Caucasian languages than to most Indo-European languages. South Caucasian is a weaker type III, but also has a strong verb final rule. The unusual consistency of type III in Hittite, in particular, probably reflects the length of its separation from an ancestral type II system of PIE, and also the long term interaction with speakers of northwestern and southern Caucasian languages. Weight must be attached to the influence on Hittite, and, more specifically, on the Hittite scribes, of the Sumerian and Akkadian languages, both of which are verb final. Just as the Hittite vocabulary is flooded with words from these languages, so Hittite syntax must have been deeply influenced; one leading Hittitologist has in fact called the result a creolization (Ivanov 1963:128).[13]

Italic was a moderate type III language, the only consistent type III feature being the verb final rule in Old and earlier Classical Latin. The fully developed type II of later Romance may reflect, not the emergence of something entirely new, but the recrudescence and strengthening of colloquial patterns of great antiquity.

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[13] The culturally superior Hattic peoples were the *eventual* substratum in the Hittite imperial area, but Hattic itself remains poorly known, and the degree to which an influence on Hittite can be or has been demonstrated remains rather limited (see Berman's attempt 1972). The tendency in some quarters to derive many Hittite patterns from vague or completely speculative Hattic or Hurrian originals is an excess of the *lux ex oriente* model.

Most of the strongest evidence for SOV thus comes from four stocks that are part of a Eurasian syntactic sprachbund (Masica 1972). I find it significant that not one of these eastern stocks is of the *rigid* type III, although Dravidian, Altaic, and northwest Caucasian are. If the early Indo-European languages *had* been of type II and had then spread into type III areas, one would expect type II in the central or homeland area and non-rigid type III's in the languages that entered type III areas — which is exactly what we do find. If, on the other hand, ancestral languages of a rigid type III had spread into rigid type III areas, one would expect rigid type III's in both the center and the margins, which is precisely what we do *not* ever find. This picture of the evolution of PIE syntax types in relation to those of surrounding and interacting systems adds another strong argument in favor of an original type II.

Turning to the west, the typologically extreme and consistent version of type I that is exemplified by Old Irish and Celtic generally probably reflects drastic restructuring due to interaction with a pre-existing population speaking Proto-Berber or some other early Hamitic or Western Afro-European language, that was also spoken in the British Isles (Wagner 1967).[14]

4. Conclusions

4.1. *Methodology.* The problem of "PIE as II" has now been considered in terms of syntactic theory, the historical-comparative method, and of typology. I have been particularly careful to relate the problem of PIE syntax to the generalizations by Greenberg. And this careful reticulation has raised at least four more general, methodological considerations.

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[14] The Celts were probably in the western part of the homeland area. The substratum hypothesis involving a Proto-Berber or Proto-Hamitic language is made probable by three interrelated facts: (1) their typological closeness to Proto-Celtic, (2) the presence of such Berber and Hamitic communities in Northwest Africa during the period in question, and since, and (3) the extraordinary typological difference between Celtic and the other Indo-European languages.

Many of the type II patterns and rules contribute substantially to the case for a type I in PIE. In the first place, any good argument for SVO also contributes to VSO unless there is a specific conflict regarding the relative ordering of S and V. In the second place, there is the VSO patterning in both Armenian and Slavic, the high frequency of marked verb-initial alternates in several Germanic dialects, the functions of verb-initial orders in Hittite, Latin, and elsewhere, and also the various verb-initial alternates for relative clauses. Third, there are the preposition-pronoun orders in Old Hittite, the productivity of VS compounding in several stocks, and the initial position of interrogatives, and the fact that inflected auxiliaries precede and do not follow the verb, and so forth (see particularly Miller 1975). Fourth, the foregoing arguments for modified-modifier order in NG, AS, the postposing of relative clauses, and the preposing of locative auxiliaries can all be used in an argument for the type I hypothesis. Some interesting theoretical arguments occur in McCawley 1970.

First, diachronic and typological discussions of syntax continue to suffer from a general atomicity and the unprincipled forefronting of the three variables, S, V, and O, and their interrelations. While the 'verb', by most definitions, is the most powerful unit variable, it is part — and often a dependent part — of a much larger system. Greenberg did syntactic studies an inestimable service by creating a typology of gestalts. While certainly derivative from Sapir and Jakobson, it adds many new components in the systematic conceptualization of 'harmonics' and the 'topic-comment' model — and many empirical refinements. Diachronic and typological syntax of the future, rather than reverting to atomism, should operate in terms of such constructs and additional ones, such as the rigid type III, the disharmonic but stable subtypes of II, and so forth.

Second, many current syntactic studies tend to operate 'typologically' in the sense of bolstering a specific hypothesis by ad hoc comparisons to privileged non-Indo-European structures — conspicuously Turkish and Japanese in the arguments for the type III hypothesis for PIE. This approach, which I have called 'triangulation' (1974:34) can yield deep insights, particularly when the linguist has relatively deep intuitions of the structures in question (e.g., Bloomfield ref. Sanskrit and Algonkian). On the other hand, I would urge that an equally and possibly much more significant use of typology is in terms of our growing set of generalizations based on an adequate sample of languages and formalized in terms of unidirectional implications and a probabilistic model of validity. Universals of this Greenberg form constitute indispensible constraints and guidelines for reconstruction. The differences between such a probabilistic use of typology and the more intuitive and qualitative character of triangulation is far too often ignored in recent statements.

Third, students of diachronic syntax tend to base their work excessively on the impressionistic pronouncements of authorities. Within Indo-European, of course, this reflects the complexity and massiveness of the relevant data and scholarship, and the fact that no one knows well more than a few of the syntaxes in question. Simple counts of an adequate sample are much better than nothing, but these counts should not obscure the fact that other and more sophisticated parameters of degrees of markedness are our ultimate goal. Such parameters have been partly defined by Greenberg (1966) but a great deal of general and language specific conceptualization remains to be done — with particular reference to syntax. There is a widespread tendency to deplore the poetic character of many of our best early texts, notably the Rig Veda, Homeric Greek, and Old Irish. But it is precisely such texts, rather than, for example, Hittite prose, which will enable us to work out a suitably refined analysis of *degrees* of markedness.

Fourth and last, syntactic reconstruction currently depends all too heavily on the generalizations which Greenberg hammered out well over a decade ago, to which little has been added since. Future reconstruction will depend on elaborating the dozens of additional 'empirical universals' which are waiting to be stated. The empirical inadequacies of our Indo-European syntax, in particular, are indexed by the almost total silence on Old Armenian, Tocharian, and so forth, and the dependence on egregiously antiquated sources such as Berneker. For the future we need systematization of the already rather large body of 'laws' and other rules of PIE syntax on which there is a scholarly consensus — such as 'Behagel's Law' (regarding the suffixation of complex subordinate clauses), and 'Wackernagel's Law' (regarding the unstressed second position). Further clarification of the order of meaningful elements in PIE will depend on the updating and systematization of syntaxes of the given languages, and on a much larger framework of Greenberg-type universals.

4.2. *Specific conclusions regarding 'the devil's case'.* I would conclude that the evidence is very strong that PIE had SV and AN orders, but would note the also powerful arguments that the latter of these can combine and coexist about equally well with either VO or OV. Otherwise, the case is good for the postposing of relative clauses, and fairly good for AS in comparison, the preposing of locative auxiliaries, NG over GN, and for *either* SVO *or* SOV.

The highly developed case system of PIE, with its eight underlying and surface-morphological categories, is something about which most scholars agree in its essentials — even ardent proponents of the type III hypothesis. A case system of this sort is congruous with and typical of a loose, partly disharmonic type II structure (although the degree to which it is *more* congruous with this than a rigid type III remains to be investigated).

These and yet other arguments, then, support the hypothesis of OV in PIE, and more generally, of type II.

The early dialects evolving from PIE shifted to type III in the east as a result of language contact, sometimes lasting over millennia, and variously involving Caucasian, Finno-Ugric, Altaic, and Dravidian. In the west, Celtic shifted to a radically different type I after similar influence from western Afro-European languages (Proto-Berber?). At different times between about 3,500 and 2,000 B.C. or even later there took place the *syntactic trichotomization* that has perdured ever since.

The present solution has not been argued explicitly in terms of parsimony, but does contribute to a prehistory of Indo-European syntax that would be simpler than the proposed alternatives; the Celtic transformation mainly calls for fronting the verb, and shifting to NA; the four eastern stocks go through several Eastern transformations. The proposed solution is vastly simpler in terms of ordered, diachronic transformations than, for example, going from a type I *Ursprache*, to a partial type II in eleven of the twelve stocks, to type III in five of the latter, and then back to type II or partial type II in some of *these* — and yet other, imaginable prolixities. The type II hypothesis advanced above results, in short, in a diachrony that is more parsimonious.

A related conclusion is that it has been fallacious not to recognize the diversity of the syntactic prehistories and histories of the stocks. The Celtic and quite distinct eastern transformations have just been discussed. To take one more example, we have Armenian and Slavic shifting toward the near equality of SVO and VSO, before going on to SOV and SVO, respectively (but to SOV in West Slavic). Italic is a rather special case, shifting from the type II of PIE to a weak type III (with a strong verb final rule), and then back again to a type II in late Latin and the Romance dialects. It is natural that a dozen stocks spread over three continents and four millennia should have had drastically different syntactic histories.

To sum up, to the extent that the evidence permits — and this extent is limited — PIE was type II or, to be somewhat more precise, exemplified one, imperfect variant of Greenberg's ideal type II. The devil's case rests.

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On the SOV Origin of the Suffixal Agreement Conjugation in Indo-European and Semitic

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1. Introduction

The typological observations made by Greenberg in his pioneering article (1966) concerning the relationship between morphology and syntax have served as a departure point for a methodology of internal reconstruction in diachronic syntax in which it is recognized that the frozen order of bound morphemes reflects in some fashion the syntactic order prevailing at the time the erstwhile free morphemes became bound. This methodology, which recognizes the more conservative status of bound morphology vis-a-vis syntax (see e.g. Givón 1971, 1974), may actually be traced back at least to Bopp (1833). Greenberg's observations, in particular those concerning the statistical correlation between OV syntax and suffixal morphology as against VO syntax and prefixation, have also given rise — inadvertently — to a number of misunderstandings concerning the precise nature of the causal relationship between syntax and morphotactics. I have attempted elsewhere (Givón 1971) to show that this relationship is best understood in terms of the diachronic rise of bound morphemes from free lexical morphemes, rather than in terms of analogy. And that further, the typological consistency of languages is forever being subverted by ongoing syntactic change, so that only languages which have maintained their word-order typology constant for a great number of years, probably for a number of millennia, show complete consistency between morphology and syntax and thus conform to the statistical tendencies observed by Greenberg. Conversely, an inconsistency between morphology and syntax is the best evidence — when used judiciously — for diachronic syntactic change. Evidence of this type, among others, has lead Lehmann (1969) to reconstruct an SOV stage somewhere in the pre-history of Indo-European. In this paper I propose to deal first with one morphemic phenomenon which has been previously considered problematic for the SOV reconstruction of Indo-European, namely the suffixal subject-agreement conjugation of verbs. I am going to suggest that far from being damaging to the SOV reconstruction, this phenomenon is actually quite consistent with it. I am going to further show that a similar phenomenon must have occurred in Semitic; and in order to justify the applicability of the Semitic case, I will also attempt to show that a sufficient body of evidence exists for reconstructing Proto-Proto-Semitic as an SOV language.

2. Subject agreement and subject pronouns

The methodology I am going to rely upon rests heavily on two assumptions which I have developed elsewhere and will only briefly sketch here:

(i) That subject agreement paradigms on verbs always arise, diachronically, from anaphoric subject pronouns that eventually become bound morphemes. Their position as bound morphemes vis-a-vis the verb stem merely reflects their free syntactic position at the time of binding. I have discussed this in great detail in Givón (1975) and have nothing further to add to that discussion.

(ii) That — all other things being equal — free pronouns, be they subject or object pronouns, tend to occupy the same syntactic position as would the full NP's of the same case function.

While I consider postulate (i) above to be totally incontrovertible, the evidence leading to propose (ii) is not as clear cut. The best support for this postulate, to my mind, involves data of languages which are in the process of renovating a pronominal series following the demise[1] of the anaphoric function of an earlier subject or object agreement. In all cases known to me, the new — independent — anaphoric pronoun appears in the same position of the full NP of the same function. Let me cite two examples for object pronouns first. Spanish (as well as other Romance languages) has evolved the pre-verbal clitic object pronouns toward an obligatory agreement system, at least for definite, topical or human objects (see discussion in Givón 1975). The pre-verbal clitics still retain their normal anaphoric function. In addition, however, the independent (subject) pronouns *el, ella, ellos, ellas* together with the dative/human preposition *a* are used *post*-verbally in emphatic/contrastive context, as in:

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[1] The demise of an agreement conjugation as a viable anaphoric device is due to phonological attrition and neutralization, much like in other bound morphemes. Current French is a good example of a language undergoing the last stages of this process. But the loss of the anaphoric function of the agreement markers, especially with respect to third person singular and plural, may come much earlier than the complete phonological erosion of the paradigm as observed by the linguist. Thus, in Israeli Hebrew, for example, the third person agreement in the past and future conjugations is quite distinct from the rest of the paradigm, yet it does not function in anaphoric contexts any more, where the independent subject pronouns *hu, hi, hem, (hen)* are obligatorily used. I suspect this is due at least in part to the fact that in one tense-aspect, the participial-present conjugation, only gender and number agreement is possible, but no person agreement. Thus in anaphora independent pronouns must be used in this paradigm, and their use may have spread into the past and future paradigm via analogy, though there it is still superfluous to some extent.

(1) *le* vi ayer 'I saw him yesterday' (anaphoric)
le vi ayer *a él* 'I saw *him* yesterday!' (not *her*) (emphatic)

The position of the new independent pronoun is consistent with the current post-verbal position of object NP's in Spanish. Virtually the same situation is occurring in a number of Bantu languages, where the clitic object pronouns were 'trapped' pre-verbally during an earlier SOV syntax (see Givón 1971, 1974). Thus Zulu presents an identical situation to that of Spanish (see Kunene 1975), while in Duala (see Epée 1975) a new object pronominal series has evolved post-verbally and the pre-verbal clitic object pronouns have disappeared altogether.

The same may be shown for subject pronouns. English anaphoric subject pronouns, which have replaced a defunct suffixal subject agreement, appear at a position consistent with the current SVO syntax of the language. Similarly in Israeli Hebrew, which is currently in the middle of drifting from a pragmatically-controlled VS-SV syntax toward a grammaticalized SVO syntax, the independent anaphoric subject pronouns, which have replaced the verb-bound subject agreement in anaphora, appear pre-verbally, in a position consistent with the preferred definite subject NP syntax (see discussion in Givón 1976a). An opposite example may be shown for Nilo-Hamitic, where the current syntax is rigidly VSO. The verbs still show a prefixal subject agreement conjugation, but the independent subject pronouns — already used obligatorily in anaphora, as in Hebrew — appear post-verbally, in a position consistent with the current VSO typology (see details in Givón 1976b).

One must nevertheless add a word of caution here. In a number of languages the syntactic order is different for definite and indefinite NP's. For example, in Mandarin (see details in Li and Thompson 1974), definite object NP's are pre-verbal while indefinite ones are post-verbal, and definite subjects are pre-verbal while indefinite subjects may often be post-verbal. In cases of this type one would expect, for obvious pragmatic reasons, the anaphoric pronoun to conform to the syntax of the definite NP rather than the indefinite. But things could even get more complicated. Both Spanish and Israeli Hebrew show a large freedom of VS-SV variation that is controlled by the pragmatics of *relative topicality* of the subject vis-a-vis the verb or other NP's (see discussion in Bolinger 1954 and Givón 1976a). This variation can involve both definite and indefinite subjects, although clearly the stronger tendency toward VS syntax, at least in Israeli Hebrew, is with indefinite subjects, and the tendency to grammaticalize the SV(O)) syntax follows the gradient: PRONOUN > DEFINITE > INDEFINITE. In both Spanish and Israeli Hebrew the SV(O)) syntax is most evident with anaphoric subject pronouns. In Spanish this tendency bears a few qualifications, but in Hebrew it is absolute. In other words, the anaphoric subject pronoun is the *pioneer* subject type to go pre-verbally in a language which is moving from VSO to SVO typology. And were the subject pronoun to cliticize at that point in time, its pre-verbal bound position will reflect the diachronic *drift* of the language at that point, rather than necessarily the actual facts of the syntactic position of the subject NP's. These are highly specific considerations and they must be borne in mind when one uses morphotactic evidence to reconstruct historical syntax. The evidence also offers a number of mitigating circumstances that in a sense cushion the impact of these cautionary considerations. For example, purely pragmatic considerations (i.e. 'more topical material tends to move forward') would militate against the tendency in both Bantu and Spanish to move object anaphoric pronouns post-verbally, but still the newly emerging object pronouns conform to the current VO syntax. Similarly, purely pragmatic grounds would militate for placing the independent anaphoric subject pronouns in Nilo-Hamitic *pre*-verbally, but in fact they appear post-verbally, conforming to the rigid VSO syntax of this group. Thus, while one must apply the methodological generalization in (ii) above with a certain amount of caution, its actual track record is reasonably attractive.

3. SOV and suffixal subject agreement

In this section I will sketch out a simple explanation for the SOV origin of (at least some) suffixal subject agreement paradigms. There is no way, I think, the case can be actually *proven* for Indo-European, since a proof hinges upon a certain etymological evidence that I don't believe is available for this group. However, in the following section I will show a parallel case in Semitic where etymological evidence of precisely the right kind is still available.

The explanation may be sketched out as follows:

(a) The most common universal source for tense-aspect morphemes in language is from a certain group of main verbs which then become 'auxiliary', often take infinitival complements, and eventually fuse to the verb stem as prefixes or suffixes.[2]

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[2] For an extensive discussion of this, see Givón (1971, 1973).

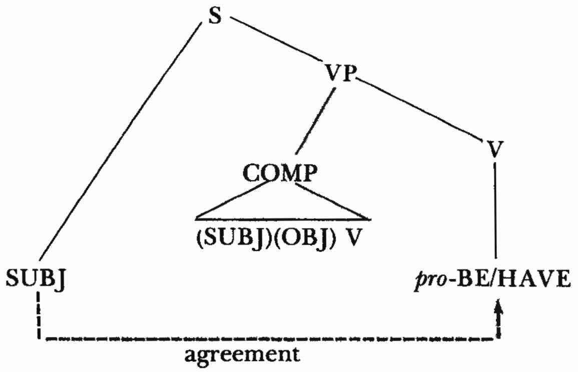
(b) Of these, the most common precursor for perfective aspects — and thus for eventual past tenses — are 'be' and 'have'.

(c) In an OV language such an auxiliary would appear *after* the main verb, the latter being its complement.

(d) In a subject-first language the subject pronominal agreement — which appears on the main-auxiliary verb rather than on its non-finite complement — will naturally appear as a *prefix* to that auxiliary verb.

Put all together in an illustrative tree diagram, one obtails:

(2)



The diagram in (2) above represents the stage where the anaphoric subject pronoun has already become an obligatory subject agreement, but before the auxiliary tense-aspect marker has become bound to the verb. If at that point the auxiliary becomes fused to the complement/non-finite verb, it would obviously — in an SOV typology of this kind — be fused to the verb stem as a suffix. And given that it already carries the subject agreement with it prefixally, that subject agreement will be now *sandwiched* between the new main verb and its suffixal tense-aspect marker. As a consequence the erstwhile prefixal subject agreement has become *suffixal*.

The next stage of such a development is quite predictable: a fast development of morpho-phonemic variation in the now-suffixal agreement conjugation, due to the erosion of the word-final tense-aspect marker and the fusion effect arising from the sandwiched position of the agreement pronoun. One would thus expect this type of a situation to produce an extensive cross-dialectal morpho-phonemic variability in the conjugation, resulting in greater difficulties of tracing the etymological relation between the subject agreement conjugation and other pronominal series in the language, especially if the other pronominal series were innovated at a later date. These two features — cross-dialect morphophonemic variability and etymological haziness — are quite characteristic of the suffixal conjugation of Indo-European. They are also characteristic of the suffixal conjugation of the perfective in Semitic, as compared to the much more regular pronominal prefixes of the imperfect. To sum up then, rather than being an inconsistency in the SOV typology of IE, the suffixal subject-agreement conjugation is actually a highly natural development consistent with SOV syntax.

4. The Semitic perfective suffixal conjugation

It has been previously assumed (see e.g. Givón 1971), I think rather hastily, that the suffixal subject agreement conjugation of Semitic is consistent with the VSO syntax of classical Hebrew, Arabic, Ethiopic and Aramaic. In this section I would like to show that this could not possibly be the case, and that it could only be consistent with an SOV syntax at the time of its inception. The argument is surprisingly straightforward:

(a) The perfective suffixal conjugation is traced back etymologically to a pronominal series of the verb 'be', where the pronoun *preceded* 'be'. It is cognate to the Akkadian nominal-stative conjugation with 'be'. (See Haupt 1878 and discussion in Hudson 1974).

(b) When a VO language innovates auxiliaries and turns them into tense-aspect markers, they appear as *prefixes* to the complement/main verb.

(c) Therefore the suffixal auxiliary 'be' of the perfective — together with a *prefixal* pronoun/agreement it carried — could not have arisen during a period of VSO syntax, nor could it have arisen during a period of SVO syntax, which would have also given rise to tense-aspect *prefixes*.

(d) Finally, the prefixal position of the pronominal series vis-a-vis BE is also inconsistent with VS syntax, where one would have expected a suffixal pronoun conjugation (as in Nilo-Hamitic, see above).

It is thus obvious that the only syntactic typology capable of giving rise to the suffixal agreement conjugation of the Semitic perfective must be SOV, as diagrammed in (2) above. In the following sections I would like to survey other facets of Semitic morphology which also point out to an earlier period of SOV syntax.

5. The SOV stage of Proto-Semitic

The classical Semitic languages such as Biblical Hebrew, Classical Arabic, Biblical Aramaic or Ethiopic (Ge'ez) and others have been traditionally described as VSO languages, and it has been tacitly assumed that Proto-Semitic was also a VSO language. It has also been assumed that the SOV syntax of Akkadian must have arisen via contact, presumably with Sumerian. In this section I would like to show that in addition to the suffixal perfective conjugation, other facets of Semitic morphology must have also arisen during a period of SOV syntax. I will also evaluate the status of Akkadian, and conclude — in contrast to a more hasty suggestion made in Givón (1975a) — that the SOV syntax of Akkadian does not represent a direct continuation of the putative early SOV stage of Proto-Semitic, and that in fact the language had already developed VO syntax prior to its attested SOV stage.

5.1. *The prefixal conjugation of the imperfect.* The imperfect conjugation of the classical Semitic languages shows a prefixal subject-agreement. This clearly goes back to Proto-Semitic, since all dialects have at least one accentual conjugation with this pronominal series, Akkadian has two (in addition to the nominal-stative suffixal conjugation which is cognate to the perfective suffixal agreement of the other dialects), and according to Moscati et al. (1964) other West-Semitic dialects may have also had two prefixal conjugations. In addition, Hamito-Semitic (Berber) and Cushitic (Somali type) also have two prefixal conjugations. Now, if postulate (ii) in section 2 above is correct, as I suggest it is, then the prefixal pronominal conjugations in Semitic could not have arisen from VS(O)) syntax, but only from a stage in which the subject *preceded* the verb. This could of course be either SOV or SVO, but given that the *suffixal* conjugation could have arisen only from an SOV stage, the SOV reconstruction is preferred. Further, while the suffixal conjugation presents monumental cross-linguistic headaches and is extremely variable and problematic, the prefixal conjugation is quite easy to reconstruct cross-dialectally and is much freer of morpho-phonemic variability (see comparative data in Moscati et al. 1964). If both arose during the same SOV period, the 'sandwich' effect of the suffixal conjugation thus explains the differences in a natural way. Otherwise one would have to assume — all other things being equal — that the suffixal conjugation is much older. But the *length* of the suffixal conjugation is usually *greater* than that of the prefixal pronouns, suggesting that it is either *younger*[3] in time or that it must have involved a more complex morphemic array, not simply a pronominal element. And this is of course consistent with the etymology of PRO-BE (rather than PRO alone).

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[3] Since all bound morphology erodes in time, the longer it has been bound, the smaller one expects the affix to be.

5.2. *Case suffixes on nouns.* Moscati et al. (1964) reconstruct the three case suffixes *-u* (NOM), *-i* (GEN) and *-a* (ACC) for Proto-Semitic, with the possible addition of *-u* (LOC) which is attested in Akkadian, perhaps vestigially in Arabic as well. The dialect distribution in the early dialects suggests:

(a) The three cases are attested in Classical Arabic, Amorite, Tel Amarna inscriptions, Ugaritic and Akkadian (there with the suffixal *-m/-n*).

(b) Frozen survival of *-a* appears in Biblical Hebrew for 'directional'. It may be also attested in frozen forms in Biblical Aramaic. The same seems to be true for Ge'ez (Ethiopic). Some vestigial survival of the nominative *-u* may perhaps appear in numerals in Ge'ez.

(c) Moscati et al. (1964) list 8 prepositions reconstructible to Proto-Semitic. Their *size* is much larger than the mere single vowel of the case suffixes, suggesting a *later* point of origin. This is also hinted by the semantic distribution of the two classes: while the case suffixes mark the main 3-4 case functions (NOM, ACC-DAT, GEN), the prefixal morphemes mark further elaborations of oblique cases (various LOCATIVE categories, INSTRUMENTAL, BENEFACTIVE). They thus bear the marks of a supplementary case-marking system. Further, for almost every one of these prepositions an etymological relation with a noun or verb root can still be postulated.[4] Such an etymological connection, particularly via NP-of-NP genitival compounds, is also hinted by the fact that many of the prepositions still govern a *genitival* case in the nouns they modify.[5]

I have suggested elsewhere that morphemes marking the case-function of nominal arguments vis-a-vis the verb arise diachronically from only two sources: serial *verb* constructions and NP-of-NP genitival *noun* compounds.[6] While this assertion may strike some people as being unprovable, I do consider this point well established so as to make further substantiation at this point superfluous. The serial-verb source of case markers normally involves non-subject cases. In these instances, the relative order of VO or OV determines whether the erstwhile serial verb becomes a *pre*position or *post*position on the noun, respectively. If suffixal case markers arise from serial verbs, they must have arisen during a period of OV syntax.[7] On the other hand, if the source of case-markers is a genitival noun compound, then the *head* of the construction is the noun that becomes a case marker. To the extent that the correlation observed by Greenberg (1966) between OV syntax and GEN-N or MOD-N order in the noun phrase is valid, the suffixal case-markers must have arisen during GEN-N syntax in the NP and thus OV syntax in the VP. While prefixal markers must have arisen during N-GEN syntax in the NP and thus VO syntax in the VP.

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[4] The prepositions *'l* 'on top' and perhaps also *ʔl* 'to' and *l* 'to' may be related to the root **'l* 'rise', 'top'. The preposition *mn* 'from' may be related to **man* 'part' via a plausible partitive derivation. The preposition *'m* 'with' shows an archaism *'md* in Hebrew and may thus be related to **'md* 'stand'. The temporal-goal preposition *'ad* 'until', 'up to' may be cognate to the Hebrew *mo-'ad* 'festival time', *ʔid-an* 'era' or **w'd/*y'd* 'assign as goal'. The preposition *b* 'in, with, at' may be cognate to the verbal root **bʔ* 'enter' > 'come'.

[5] See discussion in Givón (1971). The Akkadian data in section 5.7 below furnish a nice example of how the prepositions govern a GENITIVE case of their nouns. The genitive case in many nouns governed by prepositions in Indo-European dialects (German, Latin) probably also points out to a NP-of-NP origin of these prepositions.

[6] See discussion in Givón (1971, 1974). One case marker could of course turn into another, and this is particularly significant in the generation of *subject* case markers, for which an etymology via the serial-verb source is almost impossible to justify, and derivation via the noun-compound source is almost equally hard to justify. The most common source for NOMINATIVE cases known to me are the GENITIVE (as in Japanese, many Sino-Tibetan lan-

To sum up, then, if the case-marking suffixes of Semitic are indeed much older than the prepositions, as clearly seems to me the case, then Pre-Proto-Semitic must have undergone a stage of OV syntax before it changed into V(S)O.

5.3. *Nominalizing suffixes.* As summarized in Moscati et al. (1964), Proto-Semitic must have had a considerable array of nominalizing suffixes converting verbs to nouns, nouns to nouns or nouns to adjectives. I have discussed elsewhere the diachronic relation between nominalizing affixes and NP-of-NP genitival compounds.[9] Briefly recapitulated, the head noun in genitival compounds becomes the derivational affix. If the syntax at the time of this development is N-GEN, a derivational *prefix* develops, while if the syntax is of the GEN-N type normally associated with OV typology, the derivational affix will be a *suffix*. Let us briefly survey the

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guages, a number of Malaeo-Polynesian languages, subordinate clauses in Uto-Aztecan and Yuman languages, Turkish subordinate clauses and probably many others), the INSTRUMENTAL (as in a number of ergative languages such as North-Persian dialects) and in fewer cases also the DATIVE (as in one aspect of Neo-Aramaic, in the evolving paradigm of 'have' in Israeli Hebrew, possibly also in the dative-subject phenomena in various Indo-European dialects. See discussion in Givón, 1975). Other case-mutations are also well recorded, such as DAT > ACC, ACC > GEN, LOC > GEN, LOC > ACCOMP, ACCOMP > INST, INST > MANNER and possibly others. But since bound morphology always erodes, new sources for innovating new case markers must always be found, and those sources are lexical nouns and verbs.

[7] See discussion in Givón (1971, 1974).

[8] One must make here a certain qualification: the syntax of Noun Phrases, particularly with respect to NP-of-NP compounds which tend to become lexicalized very rapidly, is in general much more *conservative* than that of the Verb Phrase. The order of the GENITIVE modifier with respect to the head noun seems to be the most conservative feature of NP syntax. Thus case markers arising during a period of OV syntax do not necessarily reflect OV syntax at the *same* period, but may reflect OV syntax at an *earlier* time.

[9] See Givón (1971).

suffixes listed by Moscati et al. (1964). I will cite only Hebrew examples for illustration, since equivalents are presumably available for all major dialects.

(a) *-an/-on*: this suffix is found in diverse function. It may derive an adjective from a noun as in **rš* 'head' > *rišon* 'first'. However, since the adjectival form is indistinct from the nominal, the pattern is probably that of nominal derivation. It is used in deriving agentive nominals from a verbal base, as in **slḥ* > *salḥan* 'forgiving, forgiver', **rḥm* > *raḥman* 'pitying, pitier', and this derivation is probably not distinct from the 'adjectival' one. It is also found in derived object nominals, as in **qrb* 'approach' > *qorban* 'sacrifice'. It may also derive action nominals as in **ptḥ* 'open' > *pitḥon* 'opening'. It may occasionally appear in diminutive-derived nouns, as in *ʔiš* 'man' > *ʔišon* 'little man'.

(b) Moscati et al. (1964) suggest the possibility of an *-l* derivational suffix deriving nouns from nouns, as in **krm* 'vineyard' > *karmel* 'Carmel', but this may be a residual old pattern with only leftover attestation.

(c) Moscati et al. (1964) also suggest a noun-to-noun derivational suffix *-m* as in **sf* 'lip' > *safam* 'moustache', but note that this may be a relic of 'mimation'.

(d) The suffix *-i/-ay/-awi* is used extensively to derive adjectives and nouns from nouns. It may give place of origin derivations, as in *Gil'ad* > *Gil'adi* 'coming from Gil'ad'. It may give an occupational noun/adjective, as in Egyptian *ḥmw.ty* 'artist'. Since the adjectival paradigm is not really distinct from the nominal, the best guess is that this was a noun-to-noun derivation, perhaps most pertinent to human-agentive nouns deriving point of origin, occupation etc.

(e) The suffix *-ut* is used extensively in deriving abstract nominals from both verbs and nouns, as in **mlk* 'rule' > *malxut* 'kingdon, rule', **yld* 'child' > *yaldut* 'infancy', 'childhood', **rʔ* 'see' > *rʔut* 'seeing'. Moscati et al. (1964) suggest that the derivational suffix *-it* is a variant of *-ut*, but historically this may represent a merger rather than a conditioned split.

(f) While the suffix *-a, -at* is described by Moscati et al. (1964) as a feminine ending suffix, it was clearly used to derive abstract nouns from verbs, as in **mlk* 'rule', 'king' > *mluxa* 'ruling, reign, monarchy', **rpʔ* 'cure' > *rfuʔa* 'curing, medicine'. It was often used in combination with the *m-* prefixal nominalizer, as in **gp* 'devastate' > *magefa* 'devastation, plague'. The extensive variability and irregularity in the masculine-feminine gender system suggest very strongly that the noun classification of an earlier stage of Semitic was much more

> extensive and not sex-gender based.[10] Thus the so-called feminine *-a/-at*, the plural *-ot/-at* and perhaps other suffixes traditionally ascribed to the sex gender singular or plural inflection, may have in fact arisen as suffixal derivational operators of this general type.

The traditionally reconstructible order in the noun phrase in Semitic is N-GEN, with the head noun appearing in the 'construct-state' form. But if one is to take the postulate of the diachronic origin of derivational suffixes seriously, I think their presence in Semitic points out to an earlier GEN-N syntax. The diversity, haziness and overlap in their function, as well as the traces of extensive reanalysis and functional shifting, point out to the fact that this system is very old. Aside from prefixes associated with the verbal derivation (and thus carried over into the nominal system), Semitic exhibits only one nominalizing prefix, of a very general application, *m-*. It is often used in combination with the derivational suffixes to extend its range of meanings, but according to Moscati et al. (1964) the core of its function is related to deriving *location, instrumental* and *abstract* nominalization. The latter can be probably ruled out because it appears only when *m-* is combined with a derivational suffix such as *-a*, *-at* or *-ut* or *-an*. The instrumental is probably related to the agentive-participial use of *m-* in the present-participial-adjectival form of the verb, thus suggesting that perhaps the reconstructible cluster of functions was initially *location of action* and *agent or instrument of action*. The prefixal position of this operator is consistent with N-GEN syntax — if the *m-* is etymologically derived from a noun. No etymology is available for either this prefix or the suffixes discussed above, so that a firm decision cannot be made. Another possible source for *m-* is the indefinite/interrogative pronominal element *m-*, appearing in (Hebrew) *m-i* 'who', *m-a* 'what?' etc. If this pans out, it would suggest that the origin of the prefix is from a construction where it was the *head of a relative clause* in which the verb appeared in a nominalized form. The connection between relative clauses and nominalizations is far from spurious both synchronically and diachronically.[11] The prefixal position of *m-* is thus consistent with N-MODIFIER syntax, which is in turn typologically consistent with the assumed VO syntax of Semitic. The nominalizing suffixes, on the other hand, are inconsistent with this pattern, I see no reason for not assuming that they point out to an earlier stage of OV syntax.

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[10] For a general and comparative discussion, see Givón (1970).

[11] Many languages, such as Turkish, Uto-Aztecan, Yuman, Tibetan etc. have nominalized relative clauses, where the subject or other nominals appear in the GENITIVE case and the verb as a nominalized head of the construction. In Givón (1974b) I have suggested that an earlier form of Pre-Biblical-Hebrew relative clauses must have been of this type. This may well be extendable to Proto-Semitic.

5.4. *The verb derivational apparatus.* The verb-deriving operators in Semitic, such as the causative, passive, stative-middle voice, reciprocal-reflexive, are all prefixal. This is consistent with a VO typology, provided one assumes that the prefixal operators ultimately arose from main verbs. While other sources for some of these operators are possible, certainly a verbal source for at least the *causative* must be assumed.[12] Its prefixal position then must hint at development during VO syntax. The fact that the causative operator in Akkadian is also prefixal and cognate to other Semitic causatives (*š*-, ˀ*s*-, ˀ-, *h*-) is one strong indication that the OV syntax of Akkadian must have been preceded by an earlier VO stage, see discussion in section 5.7 below. To the extent that the de-transitivizing prefixes associated with the *passive-stative* and *reciprocal-reflexive* functions arose diachronically from a pronominal source, however, they may still reflect an earlier OV syntax.[13]

5.5. *The stem-internal inflections.* Since the internal-inflection layer of Semitic morphology is extremely old, probably the oldest layer of the morphology, and since no actual evidence, etymological or otherwise exists which may point out to its source, the discussion below will be pursued on purely general grounds. The internal stem variations are found in both verb and noun stems. In the verbal system they are most prominently associated with *tense-aspect* functions. In the nominal system they are associated with *nominalizing, female* or *plural* derivation, *dual* or *diminutive* derivation, as well as with the *construct-state* form of the head noun when modified by a genitival modifier.

There are a number of arguments which suggest, again, on purely general grounds, that internal morphology of this type must have arisen as *external* morphology, i.e. via affixation. First, there are known cases where affixes (especially suffixes) influence the vocalization and syncopation properties of the stem to which they are attached, then get eroded and leave an *internal* inflection as a relic of erstwhile external morphology. The Indo-European (Germanic) cases in both the nominal and verbal system are well known. Next, cases of an affix (in this case a prefix) becoming an infix via *metathesis* are also well attested, as in the Semitic *-t-* operator (reciprocal, reflexive, detransitivizer) or in Malaeo-Polynesian verb affixes. This process may result in the insertion of a syllable, a consonant and ultimately also a *reduplication* factor for consonants ('gemination') into the stem. One of the forms of internal inflection in Semitic involves consonant gemination. Now, with respect to vocalization or syncopation patterns which arise from vowel harmony with an estwhile affix, the following general considerations exist: (i) Vowel harmony is an *assimilatory* process. (ii) Assimilatory processes are overwhelmingly *regressive*, i.e. the following element influences the preceding one. All other things being equal then, internal inflection arising from vowel harmony is most likely to have arisen from erstwhile *suffixes* than from prefixes.

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[12] In instances where etymologies are available, I have never seen any other source for causative operators in the verbal system except main verbs of the type of 'make', 'cause', 'force', 'let', 'get' etc., which take a sentential-infinitival-subjunctive complement in which the subject is semantically coreferent to the object of the main verb.

[13] Detransitivizing operators of the reciprocal-reflexive type may arise from reflexive *object pronouns*, in which case their pre-verbal position reflects an OV syntax at the time of evolution. This is probably the case in Romance. Since reflexives may diachronically evolve into stative-passive function, as in Romance, Polish, possibly Finnish and perhaps others, perhaps then de-transitivizing prefixes such as passives or statives ('middle-voice') may also arise from OV syntax indirectly. But passive-stative operators may also rise from a main verb of the 'be' type, in which case their prefixal position hints at an earlier VO syntax. Finally, passive-stative detransitivizers may also arise diachronically from *subject* pronouns, as in Indonesian or some Bantu languages (see Givón 1975). In this case their position vis-a-vis the verb bears no evidence to either earlier VO or OV. Since no etymologies for all these operators are available for Proto-Semitic, a decision at this point is premature. In terms of their morphemic *size*, however, these prefixes seem to belong to the same generation of the causative prefix, though this is obviously not a decisive argument.

Another well attested channel by which external morphology may in time become internal involves the possible evolution of *stem reduplication*. Cases of this type have been demonstrated in the Niger-Congo family. Two possible patterns exist, depending on the factor of word-stress in compounds, and probably ultimately on the head-modifier relation of the two members of the compound. In the verbal system reduplication of the stem is most commonly associated with aspectual notions of repetition, continuation, distributive action etc. In the nominal system it is most commonly associated with pluralization, which is the semantic counterpart of the repetitive-continuous-distributive category for verbs. In the adjectival modification system, repetition is most commonly associated with intensification or degree quantification of the modifier. Now, taking as an example a hypothetical (though Semitic looking) case with *compound final* stress pattern, one may obtain the following natural progression:

(3)	sabara-sábara	(reduplication)
	> sabar-sábara	(vowel loss, allowance of intervocalic CC clusters)
	> sab-sábara	(loss of unstressed syllable)
	> sabbábara	(consonant assimilation)
	> sabábara	(cluster reduction)
	> sábbara	(elimination of syllable repetition)

The corresponding process for a *compound-initial* stress pattern could be:

(4)	sábara-sabara	(reduplication)
	> sábar-sabara	(vowel loss as above)
	> sábar-bara	(loss of unstressed syllable as above)
	> sábabara	(cluster reduction as above)
	> sábbara	(elimination of syllable repetition as above)

Which one of these patterns is more likely to have been involved in the diachronic rise of consonant gemination in Semitic — if any, would have depended to quite an extent on the compound stress pattern.

5.6. *Stress and affixation.* Moscati et al. (1964) avoid unequivocal reconstruction of a Proto-Semitic stress pattern, though they gravitate toward considering the mutually-consistent Arabic and Akkadian pattern as more indicative of the proto stage. If this is to be adopted, then the Semitic *word-stress* pattern involved stress attraction *forward*, toward the first long syllable or toward the word-initial position. Here again one can only argue on purely general grounds. The process by which lexical morphemes become inflectional or derivational affixes via reinterpretation of a 'syntactic construction' into a lexical compound and then a single lexical item involves two universal, mutually dependent factors:

(i) The *head* of the construction, which carries the more *generic* information, is the one which gets 'semantically bleached' into a grammatical morpheme or affix. While the *complement* (for verbs) or *modifier* (for nominal compounds), which carries the more *specific* semantic information, remains the 'stem'. That is, it does not reduce/bleach semantically and remains a 'free morpheme'.[14]

(ii) Corresponding to the semantic bleaching ('generalization') of the erstwhile head as it evolves into an affix, is its phonological bleaching or *reduction*, as compared to the greater phonological resistance of the semantically specific element. The most common feature, indeed the chief facilitator, of such phonological reduction is the *loss of stress* in the more generic member of the compound. The more specific member of the compound, on the other hand, is the one that retains the stress. As a result grammatical morphemes undergo phonological attrition much more rapidly and their relative size is usually smaller than lexical stems. There is thus a strong correlation in language between *semantic weight* and *phonological prominence*.[15]

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[14] For an extensive discussion and attempted explanation of this, see Givón (1971).

[15] This has complete parallelism at the sentential level, where the predicate or new-information element tends to carry the more prominent intonation pattern or stress, while more presupposed elements of background information are less stressed. See an excellent general discussion in Bolinger (1965).

Now, while one could not vouch that compound stress patterns and word stress patterns are always related, persistent parallels between the two nevertheless exist. Thus, English derivation and compoundation is primarily *suffixal*, reflecting an earlier GEN-N and OV syntax. And English word-stress tends on the whole to get attracted *forward*. English compound stress also gets attracted to the first element of the compound, the modifier/complement. Contrast now the difference between the compound stress pattern of English and Spanish in identical compounds:

(5) récord-player (OV)
toca-díscos (VO)

The derivation pattern in Romance has in general progressed more rapidly than that of Germanic toward a VO compound pattern, and it is perhaps not altogether a coincidence that while the word-stress pattern in Germanic tends to get attracted to more initial positions, Romance languages tend to attract the stress toward a penultimate — or when the last vowel is lost toward the ultimate — position. Now, why should there be a correlation between the compound-stress pattern and word-stress? The answer is rather obvious: many and perhaps most lexical items arise diachronically via compoundation. So that word-stress may be a more conservative relic of compound stress patterns and thus ultimately of syntact stress.

To the extent that all these general arguments are valid, as I think they indeed are, and in the absence of any arguments to the contrary, I think that if it turns out that Semitic indeed had a pattern of forward attraction of the word stress, that pattern may attest to an earlier compoundation pattern of *suffixation*. And to the extent that suffixal morphology is more characteristic of OV typology, as seems to be the case, then such a stress pattern also attests, indirectly, to an earlier stage of OV syntax.

5.7. *The status of Akkadian.* Adkadian is the only member of the classical Semitic languages to show strong OV features in its syntax, and it would have been tempting to count this as evidence for the OV stage of Proto-Semitic, as I have rather hastily done in Givón (1975). Unfortunately, there are a number of features of Akkadian morphology and derivation patterns which suggest that the OV syntax in the attested language must have evolved *after* a period of VO syntax, perhaps under the influence of a substratum, as seems to have happened in the case of the Ethiopian Semitic languages much later.[16] In this setion I would like to discuss the nature of the evidence which leads me to such conclusions. All the morphological evidence from Semitic discussed in the preceding sections is equally pertinent to Akkadian.

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[16] The most common suggestion is that Akkadian obtained its SOV syntax via contact with Sumerian, though this is by no means certain. To my knowledge a natural, internally motivated change from VO to OV syntax is exceedingly rare, to say the least. The only other case known to me is that of the Ethiopian Semitic languages such as Amharic, most likely due to the Cushitic substratum.

In both main and relative clauses of Akkadian, OV syntax for all nominal objects is the rule. Thus consider:[17]

(6) ūm-am kašp-am amḥur
today-ACC money-ACC I-received
'Today I received money'

(7) awil-um šū annikī'-am iddak
man-NOM this there-ACC will-be-killed
'This man will be killed there'

(8) awil-um ša eli-šu kišp-ī iddu
man-NOM REL upon-him magic-ACC-PL cast
'the man who cast magic upon him'

(9) awil-um alp-am ana di'āš-im īgur
man-NOM ox-ACC for threshing-GEN hired
'The man hired the ox for threshing'

(10) awil-um alp-am anna ebr-im iddin
man-NOM ox-ACC to friend-GEN gave
'The man gave the ox to the friend'

Relative modifiers (see (8)) follow the head noun, as do adjectives and genitival modifiers. This is consistent with the Semitic N-MOD pattern and thus with VO syntax. As I have noted elsewhere, the relation between OV syntax and MOD-N order in the noun phrase is not automatic, but reflects a long period of OV syntax.[18] The fact that noun modifiers follow the head in Akkadian, and in particular GENITIVE modifiers including pronouns which are bound to the noun stem, strongly suggests to me that the NP syntactic pattern must have evolved during a previous stage of VO syntax, and that the attested stage of OV syntax in Akkadian was *not* of a long duration. Thus, for adjectives:

(11) rē'ū damguti līt-am ṣabtu
shepherds good cow-ACC caught
'The good shepherds caught the cow'

And for the genitive pronoun:

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[17] All the examples from Akkadian cited here are taken from Buccellati (1970), and are hereby acknowledged with gratitude.

[18] In general, NP syntax — at least when the change is from VO to OV — is much more conservative than the VP syntax. See discussion in Givón (1971, 1974a).

(12) ṣibit-šu
grabbing-his
'his grabbing of

A strong suggestion that an earlier VO stage preceded the attested OV syntax of Akkadian is the post-verbal position of object pronouns, as is the case in all Semitic languages:

(13) iqar-um imḫaṣ-šu
wall-NOM struck-him
'The wall struck him'

The etymological relation between the genitive and accusative pronouns in Semitic is also evident in Akkadian, but this cannot be used to explain away the suffixal object pronouns, since the order N-GEN is also characteristic of a VO language.[19]

Verb complements of the 'that-S' type, following verbs of the 'say' and 'know' group, may appear either before or after the main verb. Thus:

(14) ul tidī *kima* mātum kaluša ša Marduk . . . ?
not know *that* land-NOM all of Marduk
'Don't you know that the whole land is of Marduk's . . . ?'

(15) kima numātum ša ittaqabbalu numāt-ima ul tīdī?
that furniture-NOM REL they-took furniture-my not *know*
'Don't you know that the furniture they took is my furniture?'

Complements of verbs of the type 'order', 'force', 'cause' etc. *precede* the main verb, and thus conform to the OV pattern, as in:

(16) bit-am ana awil-im al nadānam *aqbi*
house-ACC to man-GEN not giving I-*ordered*
'I ordered (them) not to give the house to the man'

Since the causative operator *š-* in Akkadian is prefixed to verbs, it could not have arisen from the attested OV syntactic pattern in (16), and must therefore represent an earlier stage of VO syntax (see fn. 12).

Complements of modal verbs such as 'want', 'finish', 'succeed', 'continue' etc. also seem to precede the main verb, conforming to an OV pattern. Thus:

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[19] The nature of this connection may have involved the generalization of an erstwhile accusative to a genitive function via V-O compounds of the type 'street-cleaner', 'ant-eating' etc. (see discussion in Givón 1971). The attested genitive suffix *-i(m)* on the full NP may represent an older layer of the morphology, especially since it conforms to an older OV pattern. The fact that *-i* also marks the ACCUSATIVE-PLURAL may hint at a possible connection ACC > GEN here as well. The marking of nouns following prepositions probably points out to the genitive-compound origin of these prepositions, see discussion earlier.

(17) šuma nukaribb-um eql-am ina zaqāp-im lā *igmur*
if gardener-NOM field-ACC to plant-GEN not *finish*
'If a gardener has not finished planting a field'

In this connection, the pre-verbal position of the *negative* operators, as in the *ul* forms in (14) and (15), the *al* form in (16) or the *lā* form in (17), must also attest to an earlier VO origin, since they are most commonly derived from negative-implicative main verbs of the 'fail', 'refuse', 'reject', 'avoid' type.[20] Further, although modal verbs seem to occupy suffixal positions vis-a-vis their complements, as in (17), Akkadian shows no trace of developing tense-aspect-modal morphemes from these verbs, as is done universally. This again attests to the fact that the OV stage may have been or relatively short duration. In comparison, Amharic, which has undergone a similar VO > OV shift presumably under substratum influence, already shows suffixal tense-aspect auxiliaries based on 'be'.[21] The duration of the Amharic-Cushitic contact is within the order of magnitude of up to 2 millennia, though probably less.

Further evidence attesting to an earlier VO stage of Akkadian syntax comes from lexical nominalization patterns, which are VO in contrast to the OV syntax of the sentential 'free' constructions. This is the exact converse of the English pattern, where the free syntactic order is VO but the frozen lexical pattern reflects the older OV stage. Lexical nominalized compounds are particularly conservative since they tend to evolve into single lexical items and be stored as such. Thus, consider the following Akkadian patterns:

(18) *S:* ina ṣibitt-i ussab (OV)
in prison-GEN he-stayed
'He stayed in prison'

NOM: wasāb ṣibitti (VO)
staying prison
'to stay in prison'

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[20] See extensive discussion of the derivation of negative markers from modal verbs in Givón (1973).

[21] Here one may again contrast Akkadian with Amharic, which in addition to suffixal aspect-tense markers has already evolved some case-marking *post*-positions. While frozen lexical nominalizations of the N-MOD type still exist in Amharic, as in *betä-mäṣhaf* [house-of book], 'school', the more free pattern has genitival modifiers *preceding* the head noun, as in *yä-astämari-w bet* [of-teacher-the house], 'the house of the teacher'. Adjectival and relative modifiers in Amharic also precede the head noun, though in Givón (1974a) I have cited evidence suggesting that at some earlier stage in the development of Amharic adjectives and relatives may have still *followed* the head noun while the VP syntax has already changed to OV.

(19) *S:* ina idi umma-nīya illakū (OV)
at side-of troops-my they-go
'They go at the side of my troops'
NOM: ālikūt did ummanīya (VO)
goer(s) side troops-my
'the goers at the side of my troops', 'helpers'

(20) *S:* ana šarr-im ikrub (OV)
for king-GEN he-prayed
'He prayed for the king'
NOM: ikrib šarr-i (VO)
prayer-of king-GEN
'prayer for the king'

(21) *S:* dull-am ippeš (OV)
work-ACC he-does
'He does work'
NOM: epīš dull-im (VO)
doer-of work-GEN
'workman'

(22) *S:* abull-am inaṣṣar (OV)
gate-ACC he-watches
'He watches the gate'
NOM: maṣṣar abull-im (VO)
watcher-of gate-GEN
'gate-keeper'

(23) *S:* napišt-am ana Adab iddin (OV)
life-ACC to Adab he-gave
'He gave life to Adab'
NOM: nādin napiš-im ana Adab (VO)
giver-of life-GEN to Adab
'The giver of life to Adab'

(24) *S:* qāt-am iṣbat (OV)
hand-ACC he-took
'He took the hand', 'he helped'
NOM: ṣābit qāt-im (VO)
taker-of hand-GEN
'helper'

(25) *S:* šarr-um qišt-am iddin (OV)
king-NOM gift-ACC gave
'The king gave a gift'

NOM: nidinti/nidin/nadān qišt-im (VO) (action nominal)
giving-of gift-GEN
'the giving of the gift'

NOM: nādin qišt-im (VO) (agentive nominal)
giver-of gift-GEN
'the giver of the gift'

(26) *S:* elippāt-i u-š-āpiš (OV)
boats-ACC-PL he-CAUSE-built
'He had (someone) build the boats'

NOM: ana š-ūpuš elippāt-i (VO)
for cause-building-of boats-GEN
'in order to build the boats', 'for building boats'

One may as well note that OV nominalizations are found in Akkadian, but they are of a less-nominal, more-sentential character, such as various complements. Thus, the purpose-complement pattern of VO nominalization in (26) above may be also rendered as an OV pattern:

(27) *S:* šamīt-am iḥuz (OV)
another-ACC he-married
'He married another (woman)'

NOM: ana šamīt-im aḥaz-im (OV)
for another-GEN marrying-GEN
'in order to marry another (woman)'

Notice that while in the more-lexical pattern (26) the nominalized verb appears in the lexical form of construct-state, as is characteristic of genitive compounds in Akkadian and Semitic, the verb in the less-lexical nominalization (27) appears in the normal pattern of GEN governed by the preposition, as is the free sentential pattern. The same variation may be seen in:

(28) *S:* dull-am ippeš (OV)
work-ACC he-does
'He does work'

NOM: epēš dull-im (VO) (lexical nominalization)
doing-of work-GEN
'the doing of a certain work'

NOM: dull-am epeš-um (OV) (less-lexical nominalization)
work-ACC doing-NOM
'the doing of a certain work'

Finally, in a less-lexical participial adverb nominalization one could also get the OV pattern:

(29) *S:* tupp-am tammar (OV)
tablet-ACC you-see
'You will see the tablet'

NOM: tupp-am īna amār-im (OV)
tablet-ACC in seeing-GEN
'in seeing the tablet'

To sum up the Akkadian evidence, there exist a number of indications that a stage of VO syntax preceded the attested OV syntax of Akkadian: the post-verbal position of object pronouns, the pre-verbal position of the causative operator, the pre-verbal position of the negative markers, the N-MOD syntax in the NP, particularly with respect to genitival modifiers, and finally the VO pattern in lexical nominalizations. It thus seems clear that the OV stage of Akkadian was not contiguous to the proposed OV stage of Pre-Proto-Semitic, and that the language has already undergone — together with the entire family — the drift from OV to VO prior to the re-introduction of OV syntax. The fact that the morphological apparatus of Akkadian shows virtually no traces of the attested, second OV syntactic stage, strongly hints that the later OV stage, presumably affected via contact, was of a relatively short duration. This is also suggested by the fact that the syntax of the NP had not changed to conform to the rather consistent OV syntax of the VP in Akkadian.

6. Summary

(i) I have suggested that there is nothing in the suffixal subject-agreement conjugation of Indo-European that is inconsistent with development during a period of OV syntax.
(ii) I have suggested that the suffixal conjugation of Semitic *must* have developed during a period of OV syntax, given its etymology as a pronominal series attached to the verb 'be'.
(iii) I have argued that the prefixal subject-pronoun conjugations in Semitic could not have arisen during a period of VS syntax, but only one of SV syntax. Therefore the only typology compatible with *both* the suffixal and prefixal conjugation in Semitic is SOV.
(iv) I have argued that a number of other morphological features of Semitic must have arisen during a period of OV syntax.

(v) I have argued that the period of OV syntax of Akkadian could not have been a direct continuation of the proposed OV stage of Pre-Proto-Semitic, but rather must have followed an earlier period of VO syntax.

(vi) It seems reasonable to assume that Semitic had a rather ancient stage of OV syntax, that the group as a whole had subsequently drifted toward VO and most likely toward VSO, and that the OV features of Akkadian and the Ethiopian Semitic languages represent an independent later development, most likely via rather intensive contact with an OV substratum.

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Inference or Presupposition?

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In *Linguistic Inquiry* 2,4 (Fall, 1971) there were two interesting papers dealing with conditional sentences — Geis and Zwicky (1971) and Karttunen (1971).[1] Geis and Zwicky argued that the "tendency of the human mind . . . to perfect conditionals to biconditionals" is what is responsible for the natural interpretation of sentences like:

(1) (their 4) If you mow the lawn, I'll give you five dollars.

as meaning 'and not otherwise'.
Some of their other examples are:

(2) (their 1) If John leans out of that window any further, he'll fall.
(3) (their 8) If you disturb me tonight, I won't let you go to the movies tomorrow.
(4) (their 11) If Chicago is in Indiana, I'm the Queen of Rumania.
(5) (their 12) If Andrew were here, Barbara would be happy.

I should like to propose a different explanation.

(A) When we impose a biconditional interpretation on these sentences (except for (4)), perhaps it is because we are trying to capture in logical notation the feeling that there is a causal relation between protasis and apodosis, i.e., that these sentences can be paraphrased as follows:

(1′) Your mowing the lawn will cause me to give you five dollars.
(2′) John's leaning out of the window too far will cause him to fall.
(3′) Your disturbing me tonight will cause me to forbid you to go to the movies tomorrow.
(5′) Andrew's presence here would make Barbara happy.

Of course, these paraphrases are insufficient for (1′)-(3′) in that they do not make clear that the mowing, leaning and disturbing have not yet happened, but otherwise, given a normal reading, they cover the same ground. But no one would ordinarily draw from these the inferences: (1'') 'Otherwise you won't get five dollars from me', (2'') 'But nothing else will', (3'') 'Otherwise nothing will make me forbid this'. I think that we might infer (5'') 'Barbara is not happy now', if not 'Nothing else would'. But the negation of both subject and predicate does now seem valid:

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[1] I must acknowledge the help of F. Roger Higgins in preparing this article. Boër and Lycan (1973) have some excellent remarks on Geis and Zwicky.

(1''') Failure to mow the lawn will not cause me to give you five dollars.
(2''') John's not leaning out too far won't cause him to fall.
(3''') Your not disturbing me won't make me forbid the movies.
(5''') Andrew's absence wouldn't make Barbara happy.

(B) When sentences are printed, they may be ambiguous representations for two or more distinct spoken sentences. (1)-(5) may be read so as to imply clearly that the apodosis is likely to be true under other conditions as well, by putting the high stress and tone ('focus') on a word in the protasis (*lawn, window, disturb, Indiana, Andrew*, for instance; others are also possible), following it with a sharp drop in pitch, saying the whole apodosis fast and on a low pitch with no strong stress. With this reading, which we may call the 'for instance' reading, since the protasis claims to specify only one instance of conditions which might lead to the apodosis becoming true, it is *not* subject to any tendency to 'perfect'; a biconditional interpretation is flatly impossible. But if they are read with highest pitch and stress on some part of the apodosis (e.g. *dollars, fall, tomorrow, Rumania, happy*), with another lower, falling high on some word in the protasis (maybe *lawn, further, tonight, Indiana, Andrew*), then the only natural interpretation is the causal one (only in some figurative way for (4)), which can sometimes be suggested by the biconditional interpretation. One can also convey the force of this interpretation by adding an explicit phrase or paraphrasing the conclusion so that it contains an *it* or a *that*: for (1) 'for it' or 'for doing it', (2) 'it'll make him fall', (3) 'it'll make me forbid the movies', (4) 'that makes me', (5) 'to have him'. All these seem to come under the exception specified by Karttunen (1971) in his note 1 (p. 566), since now the conclusions contain a pronominal reference which presupposes the truth of the protasis.

Not all conditional sentences are causal in type. Sentence (4) above is one clear example; Lilje (1972) cites four more all of another type, e.g.

(6) (his (a)) If it doesn't say 'Goodyear', it isn't Polyglas.

All these are what are traditionally called 'real' conditions — present real with present tenses in both parts (as in my (4) and Lilje's (a), (b) and (d)), or past real with past tenses in both parts. Such sentences, in which there is no time priority of the protasis, cannot bear a causal interpretation except in a very special way:[2]

(6′) The lack of the name 'Goodyear' causes you to know that it isn't Polyglas.

In other words the relation is evidential. And, as Lilje shows, they are not 'perfectible' to biconditionals, though they are easily converted by reversing the function of the two clauses:

(6″) If it is Polyglas, then it says 'Goodyear'.

Some future and unreal conditions may also be evidential, and some present and past real ones may be causal, but, generally speaking, this is not the case. If we wish to suggest a causal relation in present or past time, we express it with *because*, not *if*. 'If' at least pretends ignorance (except in unreal conditions), whereas 'because' claims certainty.

Geis and Zwicky's thesis is used by Karttunen (1971) to explain Lakoff's (1970) error in saying that in unreal ('counterfactual') conditional sentences, both the protasis ('antecedent', 'condition') and the apodosis ('consequent', 'conclusion') are presupposed to be false. (Of course the propositions presupposed to be false cannot be the *same* as the clauses in the conditional sentence; they are sentences made by substituting ordinary indicative tenses for the ones which mark unreality). Karttunen's sentence (1) is

(7) If Harry had known that Sheila survived, he would have gone home.

Karttunen says that he intends to show that this sentence does not presuppose

(8) Harry did not go home.

(Let's add 'at noon yesterday' to both (7) and (8) to avoid one possible quibble). Suppose we look at two different spoken sentences which are ambiguously represented by (7). Variant (a) (equivalent to *even if*) will have only one high pitch (on *had*, most likely) with fall and low thereafter. Can anyone possibly utter that sentence and then go on "but he didn't"? I can't. Clearly what is presupposed here is

(9) Harry went home (at noon yesterday).

I cannot see this as anything weaker than presupposition.[3] Variant (b), on the other hand, will have the highest pitch on 'home'. Here again, I can see no possible hesitation; this surely presupposes (8). Add 'which he did anyway' at the end of this (Karttunen's (3)), and you get something which is surely "inconsistent, if not incoherent".

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[2] Lilje's third example, "If you scratched on the eight-ball, then you lost the game", can be regarded (by non-philosophers, at least) as causal — "Scratching on the eight-ball caused you to lose the game". The opposing claim is that scratching on the eight-ball doesn't just *cause* loss of the game, it *is* loss of the game.

[3] It makes little difference, as far as I can tell, whether you take me to mean logical or

Karttunen considers Lakoff's examples with "qualifying phrases", as well as Lakoff's explanation that these phrases "cancel" the presupposition. As a matter of fact, however, they do not merely cancel, they *reverse* the presupposition. Some of these, too, have specific intonational requirements. Suppose we add *still* to (7) (after *would*), yielding Karttunen's (4), or add *anyway* at the end. Then (a) these words themselves must (normally) take the emphatic high, and (b) 'He went home' is clearly presupposed. Much the same effect may be achieved by prefixing *even* at the beginning. This time the normal intonation is the one I have described as variant (a) above, and the presupposition is unmistakably 'he went home'. Adding 'but he didn't' is surely always contradictory. Whether one adds *even* or merely alters the intonation, the result is a new construction with different properties.

Reversing the order of protasis and apodosis sometimes makes sentences of this type more natural, and this is the arrangement in which I first encountered the intonation contrast.

In Householder (1957), in the course of proposing a new analysis of English intonation, I cited some examples of intonational minimal pairs (taken in modified form from Edwards [1867]):

(10) a. The dog would haveˇdied if theyˆhadn't cut off his head.
b. The dog would haveˆdied if they hadn't cut off hisˆhead.

(This notation indicates that in (10a) there is a rise on *died* and high fall on *hadn't*, continuing to the end; in (10b) there is a high fall on *died* and a medium pitch short fall on *head*). Obviously sentence (10b) is a joke (which can be repaired by substituting *leg* for *head*), since it presupposes that they *did* cut off the dog's head, and that it didn't die. Sentence (10a), on the other hand, presupposes that the dog died.

(11) a. If a social drinkerˆdidn't become a lushˇ, heˆwould lose hisˆ healthˆ.
b. If a social drinker didn't become aˆlush, he would lose hisˆ healthˆ.

(The notation here means, for (11a) high fall on *didn't*, low rise on *lush*, high fall on *would* and mid falling to low on *health*; for (11b) high falling to mid on *lush*, mid falling to low on *health*). Here sentence (11a)

pragmatic presupposition. I am not at all convinced by Kempson's arguments (1973, 1975) that there are no presuppositions in either sense.

says that even if you avoid becoming a lush you'll lose your health anyway, while (11b) says that becoming a lush is the way to preserve health. Without the help of intonation, the distinction can be conveyed in writing by inserting *even* before *if* in the (a) sentences, or by substituting *unless* for *if . . . not* in the (b) sentences.

Karttunen's example (1) which can be made clear by *anyway* or *still* can also be disambiguated by the intonation. Note that our example (10), with protasis last, is easier to intone appropriately than (11), with protasis first. (If (11) is rearranged, it can be given the same intonation as (10), of course). But these intonational presuppositions are no different in future simple conditions than they are in unreal ('counterfactual') ones. Reverse the order of example (1) and give it the intonation of (10a) and the meaning is that I'll pay you five dollars, in any case. Or change the *would have* and *hadn't* of (10a) and (10b) to *would* and *didn't*, or to *will* and *don't* and the presuppositions change only in time. Or make my (10) or (11) present general:

(10) c. Dogs die if you don't cut off their heads.
(11) c. Social drinkers become lushes if they don't lose their health.

We still have the same opposed intonations with the same interpretations. The original form of my example (11) was future (*doesn't* and *will*); changing it to past unreal shows that general conditions sound very odd in past unreal form, except as shifted past in secondary sequence:

(12) c. (It was their belief that) if a social drinker hadn't become a lush, he would have lost his health.

In the case of a future simple condition, there can hardly be a clear presupposition of truth or falsity (since truth does not apply to the future). But there clearly is a 'not yet true' presupposition (regardless of intonation) in sentences like

(13) If Harry learns that Sheila survived, he will go home.

That is, it is presupposed that

(14) a. Harry has not yet learned that Sheila survived.

If you say, Aha! what about sentences like

(15) If it's still raining in five minutes, I'll go home.

in which it cannot be presupposed that it is not raining now. Of course, but it *is* true that it is 'not yet the case that it is raining in five minutes'. And this sentence, too, can be read with either intonation, hence either interpretation (*in any case* vs. *only if*).

None of these types, then, can be said to have open conclusions when spoken; they are ambiguous only on paper. Any attempt to preserve the ambiguity in speaking yields an unnatural sounding utterance; if you said it by accident, you'd immediately say it over with a proper intonation. I can see that philosophers are entitled to talk about printed English; but are linguists? This seems analogous to saying that 'Lead in the boats' is ambiguous between [led], the metal, and [li:d], a command to come first. The proper intonation is as much an essential part of any sentence as any word or segment in it. I am not now prepared to say how this distinction is to be introduced in the semantic representation; but it must be done somehow. Intonation is not an extraneous clue like facial expression or gestures.

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Objective Case and Agentive Case in Classical Chinese

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Based on the functional notion of case as characterized in Kao and Obenchain (1975), we can determine cases in Classical Chinese (CC). In this paper I shall confine myself to the two major cases in CC, namely *Objective* (Fillmore 1968) and *Agentive* (Halliday 1970), and propose that the objective case is primary and the agentive case is derived.

This functional characterization of the two major cases in CC differs in an essential way from other characterizations which take the agentive case as the primary case.

The claim that the objective case is the primary case in CC requires empirical evidence. The terms underlined in the following expressions selected from the book *Tso Chuan* are identified as examples of the objective case.

(S.i.1) 沙鹿崩（僖公十四年）
Sha-lu fell down. (Sha-lu had a landslide.)

(S.i.2) 平王崩（隱公三年）
King P'ing died.

(S.e.1) 星隕如雨（莊公七年）
Meteors fell like rain.

(S.e.2) 巢隕諸樊（襄公三十一年）
At Ch'ao (you) lost *Chu-fan*.

(S.d.1) 京叛大叔段（隱公元年）
Ching revolted against T'ai-shu Tuan.

(S.d.2) 衆叛（隱公四年）
The *multitude* turned against (him).

(S.t.1) 衛州吁弑桓公(隱公四年)
Chou-yu of Wei assassinated *Duke Huan*.

(M.d.1) 吾視其轍亂(莊公十年)
I perceived the ruts of their chariots were in confusion.

(M.d.2) 猶視(襄公十九年)
(*He*) was still staring.

(M.d.3) 遂惡之(隱公元年)
Then, (*Chiang-shih*) loathed him.

(M.d.4) 鄭伯由是始惡于王(莊公二十一年)
Thereupon, *Earl of Cheng* hated the king.

(M.d.5) 紀人知之(桓公五年)
The people of Ke knew it.

(M.d.6) 寡君不知(昭公十六年)
Our king did not know.

(M.t.1) 宋華父督見孔父之妻於路(桓公元年)
Hua-fu Tu of Sung saw the wife of K'ung-fu on the way.

(V.d.1) 公語之故,且告之悔(隱公元年)
The *duke* spoke to him about the reason and told him that he was repentant.

(V.d.2) 不告而退(宣公二年)
(*Ling Che*) did not answer but withdrew.

(V.t.1) 君子謂鄭莊公失政刑矣(隱公十一年)
A *superior man* would say that Duke Chuang of Cheng had failed in the administration of his government and justice.

(R–EX. e.1) 鄭有人焉(莊公二十八年)
There are (able) *men* in Cheng.

(R–P. d.1) 仲子生而有文在其手(隱公元年)
Chung-tzu was born with (remarkable) lines on her hands.

(R–A. d.1) 美而無子（隱公三年）
(*She*) was beautiful but childless.

(R–EQ. t.1) 畢萬為右（閔公元年）
Pi Wan acted as the right-hand spearman.

The parenthesis before each example in the above contain a set of codes used to simplify our discussion. The capital letters stand for types of semantic process, the small letters for classes of verbs and the numbers for numbering examples under each category.

Types of semantic process identified in CC are (S) Substantial, (M) Mental, (V) Verbal, and (R) Relational. Under relational, there are four subtypes: (EX) Existential, (P) Possessive, (A) Ascriptive, and (EQ) Equative (Halliday 1970).

Verbs in CC are classified as: (i) intransitive, (e) ergative, (d) direct, and (t) transitive (Cikoski 1970).

The distribution of the objective case with respect to types of process and classes of verbs in CC is shown in Diagrams I and II.

DIAGRAM I

Types of process	*Objective case*	*Examples*
Substantial	X	All examples with code (S)
Mental	X	All examples with code (M)
a. Perception		(M.d.1-2), (M.t.1)
b. Reaction		(M.d.3-4)
c. Cognition		(M.d.5-6)
Verbal	X	All examples with code (V) (V.d.1-2), (V.t.1)
Relational		All examples with code (R)
a. Existential	X	(R–EX.e.1)
b. Possessive	X	(R–P.D.1)
c. Ascriptive	X	(R–A.d.1)
d. Equative	X	(R–EQ.t.1)

From diagrams I and II, we can see that the objective case is related to all types of process and realized with all classes of verbs in CC. It is the only case so far identified in CC having such an overall characteristic. In other words, as the only basic and an obligatory case, the objective case is the primary case in CC.

DIAGRAM II

Classes of Verbs	*Grammatical Subject*	*Grammatical Object*	*Examples*
Intransitive	X		All examples with code (i) (S.i.1-2)
Ergative	X		All examples with code (e) (S.e.1)
		X	(S.e.2) (R–EX.e.1)
Direct	X		All examples with code (d)
Transitive	X		All examples with code (t) (M.t.1) (V.t.1)
		X	(S.t.1)

X indicates realization of the objective case.

Among the examples, there are only two that have been identified as having the agentive case as capitalized below:

(S.e.2) 巢隕諸樊

At Ch'ao (YOU) lost *Chu-fan*.

(S.t.1) 衛州吁弑桓公

CHOU-YU of Wei assassinated *'Duke Huan'*.

(Note: Examples with code (M) can also be interpreted as having agentive case in them.)

Since the terms 諸樊 'Chu-fan' and 桓公 'Duke Huan' in the above expressions have been identified and determined as having the objective case, so the terms 'you' (which can be identified from the context) and 州吁 'Chou-yu' must be in the agentive case. That is, the expression (S.e.2) 巢隕諸樊 'At Ch'ao YOU lost Chu-fan' is derived from 諸樊隕 'Chu-fan lost' by the application of an ergative transformation with zero (?) morphological modification on the surface. On the other hand, the expression (S.t.1) 衛州吁弑桓公

CHOU-YU of Wei assassinated *'Duke Huan'* is derived from 桓公死 *'Duke Huan* died' by the application of the same ergative transformation with lexicalization on the surface; that is, 弑 'assassinate' is the lexical realization of 'cause one to die' (Lyons 1968; McCawley 1971). This explains how the transitive system in CC is derived from the ergative system. We will not discuss other possible morphological modifications between the two extreme cases of zero modification and lexicalization (Chou 1962).

It is interesting to note here in passing that this characterization of the two major cases in CC furnishes an explanation of the statistical properties of verbs found in *Tso Chuan*: Intransitive verbs 10.9%, ergative verbs 40.8%, direct verbs 31.6%, and transitive verbs 16.7% (Cikoski 1970).

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Surface and Underlying Structure in Typological Study

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Typological study has not enjoyed a high status in linguistics. Its low esteem cannot be ascribed to lack of tradition, for a typological approach to language goes back as far as Adelung, who in his *Mithridates* of 1806 characterized languages for many of the traits still recognized as distinctive, such as their comparative constructions. His work itself was the culmination of collections aimed primarily at providing samples of languages. It was followed by shorter treatises proposing basic types for languages, such as that of Friedrich von Schlegel (1808) and that of his brother August Wilhelm von Schlegel (1818). These typological publications were contemporaneous with those of Franz Bopp (1816), Rasmus Rask (1818), and Jakob Grimm (1822), that set the tone for much of subsequent linguistic activity. As a glance at Adelung's preface will show, the early typological studies were not inferior to the early historical studies in their approach to language.

Nor can the low status of typology be ascribed to its concentration on morphology, as evident still in Finck's typological publications (1909). Virtually all of the work of the dominant historical strain of linguistics in the nineteenth century was also concentrated on morphology.

And while correlations between language and culture proposed by some typologists could not fail to reflect adversely on their other conclusions, the historical linguists were not free from extravagant views. In support of such a conclusion one does not even have to look as far back as Osthoff, a founder of the neogrammarian school; as recently as 1973 the respected journal *Language* included a statement that an "increasing number of scholars are returning to the interpretation of the North and West Germanic ablauting seventh-class preterits as derived from a simplification of the original reduplicating forms" (Lloyd 1973:48), thus lending its weight in support of phonological laws that seemed credible only in earlier stages of linguistics when knowledge of phonetics was very dim. Even though the status of individual publications may be reduced by quixotic views, excessive speculation by individuals has never affected an entire field of study, whether in historical linguistics or descriptive linguistics; the recent florescence of underlying sentences has not put an end to work in descriptive syntax.

Rather than for such defects, typological study may have low prestige because of a view evident in a critique of Greenberg's essay of 1963 which was included in one of the widely cited articles of recent years (Weinreich, Labov and Herzog 1968). Himself one of the participants in the Dobbs Ferry conference of 1961 which did much to bring about the current concern with typology, Weinreich with his colleagues credits Greenberg for his "use of quantitative methods and his ability to isolate significant trends in structure"; yet the statement continues: "At the same time, one must admit that he is necessarily confined to surface structure at the lowest level of reliability which is common to the description of languages available to him" (Weinreich et al. 1968:138-39). The conclusions of typological study are accordingly assumed to be vitiated by its attention to surface structure, and not very reliable surface structure at that — a critique which would of course apply to the morphologically centered typology of Finck and his predecessors as well.

In spite of this criticism the number of constructions which correlate with selected syntactic patterns indicates a structural rank that can scarcely be dismissed as surface. Even Schmidt with his overriding concern for the genitive construction found notable correlations with its arrangement; he associated postposed genitives with postposed adjectives on the one hand, and also with VO order and prepositions. These correlations and converse arrangements in OV languages are included by Greenberg in the first 25 'universals' of his Appendix III, which deal with syntactic patterns (1966:110-13). These 25 'universals', which are now subsumed under broader generalizations, have to do with verbal as well as the nominal modifiers and government constructions noted by Schmidt, with constructions including a standard, and with complements, that is to say, with virtually all the major syntactic patterns of arrangement (Lehmann 1973a, Lehmann-Ratanajoti 1976). When typological conclusions cover such a broad scope of the syntactic patterns of arrangement, it would be little short of remarkable if their basis in surface structure should lead to rejection of a typological approach to the study of language. For we seem to be dealing not with "surface structure at the lowest level of reliability: but with fundamental principles of language that govern a considerable number of 'universals'.

In attempting to understand the broad scope of these principles we might recall that of the four syntactic processes: arrangement, selection, modification and intonation, arrangement or order is the most fundamental (Sapir 1921:117). Selection classes, especially those determined by morphological markers, are less characteristic inasmuch as various expedients are used for them, notably inflections which may simply be archaic freight in a language; the *ā, o, i, u* classes of nouns and the various consonant stems of the Indo-European dialects, for example, make up selection classes which have virtually nothing to do any longer with underlying structure or meaning categories. Similarly, the semantic basis of the various conjugations must be reconstructed from chance relics in the dialects. One might justly downgrade a typology drawing on such selectional devices. The process of modification or sandhi is even less related to patterns of meaning; probably for this reason Brugmann did not even include it among the syntactic processes (1904:677-80). Intonation too follows surface constraints, as examination of a comprehensive treatment for any language will disclose, such as Pike's for English (1946). By comparison with the other syntactic processes then, arrangement or order is clearly the predominant. A typology based on it, even when surface structures are considered, may therefore have greater validity than earlier typologies which used differences in selection for their classification.

We might well ask why arrangement of surface elements leads to a more valid classification, and if there is any principle of language which would bring about relatively close agreement between the arrangement of surface structures and underlying form. The answer may be given in Behaghel's first or primary law of syntax, that is, what is closely related in meaning is also placed closely together (oberste Gesetz: dass das geistig eng Zusammengehörige auch eng zusammengestellt wird. Behaghel 1932:4).

The examples which Behaghel cites in support of this 'law' include the construction made up of substantive and genitive, that of substantive with adjective, and so on. Behaghel also states that essential qualifiers of the verb are more closely joined to it than are non-essential. If with Behaghel we recognize such a law in language, surface arrangements would parallel underlying arrangements. Accordingly, surface syntactic patterns of arrangement would not be haphazard, nor simply characteristics with which one might construct a superficial typology, but rather the patterns which provide a fundamental typology of language.

This view was represented in "A structural principle of language" (Lehmann 1973a). The principle proposed there assumes that the most fundamental relationship is found in the construction consisting of verb and its object, and further that other patterns of arrangement correlate with the arrangement of this construction. Among such correlating patterns, or typological stigmata, are those of adpositions, and comparisons of inequality, with which other patterns involving a standard are to be associated, such as names with titles, given names with surnames, and numerals in the teens. Moreover, modifiers, whether of verbs or nouns, are arranged close to the elements they modify, but in such a way that they will not disrupt the VO or OV segment. All of these constructions then are paralleled by surface patterns, and are governed by a principle regulating surface as well as underlying patterns. Moreover, Behaghel's law not only applies to the normal constructions in language but also to their disruptions through the forces of marking and of change.

In marking virtually any syntactic arrangement may be modified to bring about a difference in meaning or to achieve a special effect, whether in poetry, rhetorical usage, or everyday patterns. The use of descriptive adjectives with nouns in French provides an excellent example. Normally postposed after nouns, they are placed before nouns to achieve special meanings; thus *cher* in the common expression *cher ami* 'dear friend' differs from its usual meaning as exemplified in *un livre cher* 'an expensive book'. And the preposing of *sacre* 'holy' to *nom (de Dieu)* has such a striking rhetorical effect that it was tabooed, while in *le sacre collège* it singles out a specific 'college' — that making up the body of cardinals of the Roman Catholic church. As an example of the use of adjectives to achieve a special poetic effect we may cite Mallarmé's poem 'Sainte'. In the last stanza the 'old' sandlewood harp (*le vieux santal*) and the old book (*le vieux livre*) with preposed adjectives are parts of a vision. By contrast, in the opening stanzas the postposed adjectives — *le santal vieux . . . le livre vieux* — describe parts of the concrete scene which Mallarmé sketches. Such deliberate modifications resulting from the widespread use of marking in language require linguists to interpret texts sensitively, especially texts in earlier stages of languages; most such texts have survived because of literary or rhetorical qualities which frequently depend on marking. Ingenuous use of statistics, as in charts listing the percentage of VO versus OV patterns, or NG versus GN and the like in a given text or period, is accordingly very misleading. Yet whether marked or unmarked, the patterns concerned accord with Behaghel's first law, as the examples from Mallarmé illustrate.

Besides their modification by marking, typological stigmata may come to be modified in accordance with changing patterns in a language. French again provides an example. A group of common adjectives is not normally postposed, such as *bon* 'good', *mauvais* 'bad', *gros* 'big', *petit* 'small', *long* 'long', *court* 'short'. Their arrangement can be accounted for in much the way irregular morphological patterns of common items are accounted for; children acquire them at such an early age that the usual processes of leveling are not applied, as in such common examples as NE *man : men, good : better : best, go : went : gone*. Syntactic change is effected only gradually in language, even more gradually than phonological change, which can be brought about in the course of several generations (see the references in Lehmann 1973b, especially Gauchat 1905 and Hermann 1929). A set of studies on the syntactic change involved in the arrangement of the English genitive construction indicates that a millennium elapsed before a 1:9 ratio of postposed genitives was changed to a 9:1 ratio (Fries 1940). Yet throughout the period of this change the genitive was placed close to its head noun. Similar data could be cited for other changing constructions. Accordingly, when modifications are brought about by linguistic change, as by marking, the validity of Behaghel's first law is not undermined.

Typological classification based on arrangement accordingly makes use of characteristics of language which are regulated by meaning, not simply by conventions ascribable to the conservatism of speakers or to chance. Whether an adjective is placed before or after a noun, it makes up a unit construction with that noun, carrying through its placement a specific function. Similarly the other typological stigmata. For this reason arrangement, even at the surface, may be used for secure typological classification.

Yet what of other constituents used in earlier typological classification but not discussed above, notably the subject, as in SOV, SVO, VSO formulae and the like? In the course of recent typological study it has been noted that S (subject) is not a primary constituent; its inclusion by some linguists in the primary formulae for languages is unfortunately another instance of linguistic ethnocentrism. By contrast with the evaluation of the subject in western linguistics recent studies have shown that a subject cannot be posited for many languages, notably OV languages; instead, a segment described as "topic" of a clause must be assumed (Li 1976).

The languages in which subjects are characteristic constituents of the simple clause are the SVO languages and also the VSO languages. In VSO languages the central VO constituent is interrupted, a pattern that might well be interpreted as contrary to Behaghel's first law. Especially in view of this problem, but also other problems with the basic patterns, languages with clauses including a subject must be accounted for if the 'law' is to maintain its validity. The two proposed types, SVO and VSO, accordingly require examination with respect to the law.

In SVO languages the VO constituent is not interrupted; but these languages provide problems for verbal modifiers, like interrogative, negative, and the modalities proper. In accordance with Behaghel's law these should be contiguous to the verb. Since such an arrangement would separate the S and V, various means have been introduced to permit subjects to stand close to their verbs. For interrogative, distinct patterns of intonation are commonly used, as in English, in many Indo-European languages, and in languages generally. The arrangement of S and V then can be in keeping with Behaghel's law. For negation, many SVO languages such as French, Polish and Thai, place a negative marker directly before the verb, after the subject. German on the other hand places it after the verb, and usually after the object as well, a pattern in keeping with other OV syntactic patterns in German. English, and to some extent the remaining Germanic languages, have developed a device which permits preposing of the interrogative and negative markers while attaching them to a form of the verb; the device is the use of auxiliaries, of which the dummy verb *do* in English is particularly notable. For in providing a means to attach verbal modifiers to a verbal element and in this way permitting them to precede the principal verb without disrupting the close relationship of subject and verb, it indicates the strong tendency in languages to follow Behaghel's first law. On the other hand, languages like French, Polish and Thai, in which qualifier markers like the negative are placed between the subject and verb, have not completed the syntactic change which would bring about ideal patterns of the new type.

Even more serious disregard of the tendency to place objects next to verbs is found in VSO languages, which by their basic clause structure disrupt the VO constituent. If Behaghel's law has the validity proposed for it, one might expect some device for maintaining the verb-initial position while not leading to a disruption of the relationship between a verb and its underlying object. This device is the passive construction. Specimens of VSO languages are the Celtic, the Afro-Asiatic, the Polynesian, and some Amerindian, such as Squamish and Shuswap. The extent of development of passive constructions among them is remarkably great, so great that in a monograph on the passive Hans Hartmann related the prevalence of the construction with a particular "psychic structure" (Hartmann 1954). He found the use of the passive particularly common in the first three language groups noted above.

Without drawing the further conclusions of Hartmann concerning psychic structure, but in support of the view that the passive is almost distinctively a VSO pattern, we may cite a characterization of the use of the passive in Tagalog, which Hartmann takes from Blake (1925:140-1) (Hartmann 1954:193):

> The use of the active and passive is not optional as in English. In any given sentence the voice of the verb depends upon the relative importance of the various elements, the most emphatic idea being made the subject of the sentence. If this is the agent of the action expressed by the verb, the active voice is employed . . . The passive voice is used when the object of the verbal

> action, or some accessory circumstance is the most emphatic element, and hence the subject, of the sentence . . . Passive constructions are far more frequent than active, in fact they may be said to be the rule, and active constructions the exception.

This passage may be supplemented by a statement of Kuipers concerning the Northwest Indian Language Shuswap: "Passive constructions are used much more frequently in Shuswap than in English" (Kuipers 1974:78). In this way the passive construction permits the underlying object in a VSO language to be placed next to the verb. The VS sequence is thus maintained, for the grammatical subject, which represents the underlying object, is contiguous to the verb. Further discussion of the passive construction in language will not be included here. It is notable however that the true passive (rather than constructions like that in Japanese labeled passive) is found particularly in VO languages (Lehmann 1974:183-84). A thorough study of passives with regard to specific linguistic types and their changes would be highly useful.

Besides the passive, Squamish has a complex device, in which the object is appositional, given like the subject in the absolutive case. Kuipers, with his customary perspicuity, gives an elegant analysis of

> the Squamish transitive predicate . . . Where S and O are specified, e.g. in VtSO /na č'ə'm?t-as ta sq°ma'i̭? ta sui̭'?a/ 'the dog bit the man', the phrase VtS is the *propos* '(it is) what it, the dog, bit', and O 'the man' is the *thème* to this *propos*, so that the whole sentence can be rendered '(it is) what the dog bit, the man' = 'the man is what (or: whom) the dog bit'. In this conception of the Squamish VtSO sentence the fact that S and O are both in the absolutive case finds a satisfactory explanation: both are 'subjects' to a 'predicate'. (Kuipers 1967:173).

These patterns may indicate that while VSO languages seem to have a basic clause pattern separating the V and the O contrary to Behaghel's first law, they also include means which modify the basic clause to accord with the law. The use of such means suggests that VSO clause structure is not as favored as are the OV and SVO structures. Further, it leads to the conclusion that arrangement in general follows Behaghel's law.

We have noted above that also in historical change Behaghel's law is observed. Among the central constructions there may seem to be one exception, the relative clause. When a language is changing from OV to VO order, relative constructions may be awkward. For they may have no separate marker in OV languages, while in VO languages such a marker is virtually mandatory — though in VSO languages it may also be nonessential. When however a language is SVO, a relative marker is virtually required. Hittite provides an excellent example of the need and the development of such a marker (Raman 1973).

At a stage when the simple preposed verb no longer provided adequate indication of relative clauses, Hittite employed the particle *kui-* in a pattern that survived in Latin as well. The *quem* clause below illustrates the transitional pattern.

implōrāre	dēbētis	ut	quam	urbem	pulcherrimam	esse	voluērunt	hanc
implore	you-must	that	which	city	most-beautiful	be	they-have-selected	it

dēfendant	(Cicero, Cat. 2.13.29 — Hale and Buck, 157)
they-will-defend	

> 'It is your duty to implore (the gods) that they defend the city which they have chosen to be the most beautiful'.

At a stage closer to an OV pattern, *urbem* would be placed where *hanc* is, and the relative modifying clause would consist simply of *pulcherrimam esse voluērunt*. At a later stage, when the Indo-European languages became increasingly VO, even the transitional relative construction as illustrated by the *quam* clause above was avoided; relative clauses came to be placed after their antecedents, as in the translation above. Adoption of a relative clause structure in which the noun-modifying clause is placed contiguous to the head noun provides an additional instance of syntactic change in accordance with Behaghel's law.

A further major difficulty for maintaining the law is caused by the frequent use of numerous modifiers with a major element, either noun or verb. When this is done with nouns, the adjectives are commonly arranged so that the one most closely associated with the noun is placed closest to it. Thus in English the placement of attributive adjectives has been assumed to "correspond to the 'natural' order of recursive qualification"; that is to say those adjectives most closely related to the noun are placed closest to it (Quirk et al. 1972:924-26). Verbal qualifiers may be accounted for similarly. They are generally in the order indicated for a small set of them as follows: Interrogative Negative . . . Causative V, or the converse, as in Japanese:

yoma-se-nai ka
'read' Caus.Neg. Int.

> 'Didn't he have (someone) read?'

Of the qualifiers listed here, Interrogative and Negative typically refer to the entire clause; they clearly belong more closely to the sentence and less closely to the verb than does the causative. Accordingly the arrangement of the causative marker is explainable by the law. In addition to the typical ordering, the frequent absorption of the causative marker in verbs supports this view, as in NE *set*, OE *settan* versus Gothic *satjan* 'cause to sit' and comparable forms in other languages.

Behaghel's law then is important for the understanding of language. Patterns of arrangement in surface structure may to be sure depart from it, but as noted above such patterns are commonly modified so that surface structures correspond to underlying structures in arrangement, and to structures that are in accord with the law.

In contrast with these observations on surface syntactic structure, linguists dealing with syntax in accordance with transformational theory have directed their attention at abstract structures, assuming that transformations are universal and that languages are to be characterized by hypothetical underlying structures (McCawley 1970; Bach 1974, for general discussion and bibliography representing this view). Psycholinguistic research has however provided little support for such a view. In a discussion on this topic Professor Ilse Lehiste noted that a review of psycholinguistic research has found no evidence for the psychological validity of transformations (Fodor et al. 1974). This negative result concerning transformations does not rule out the usefulness of assuming underlying syntactic structure. But the paraphernalia posited for relating it to surface structure apparently has little or no justification. In viewing the transformational theories and the battery of metaphoric terminology devised to accompany them, some based on the figures introduced to represent underlying structure through trees, others on older folklore, one might at first be tempted to relate transformational grammar with theories like that assuming phlogiston for combustion. A likelier comparison might be the Ptolemaic theory of the universe. At a time of inquiry based on human vision the Ptolemaic theory seemed like a reasonable hypothesis to account for the phenomena of the heavens. It still has validity, as in nature poetry, even of a sophisticated poet like Dylan Thomas. Accordingly we may not need to reject every use of the transformational theory as delineated by Chomsky (1965). But it is far more important to determine those syntactic patterns in which the surface arrangements correspond to the underlying, to distinguish them from syntactic patterns in which the two assumed structures differ, and to attempt to understand the syntactic structures of language with these insights.

Typological study has provided means for distinguishing fundamental syntactic patterns from those less characteristic of the underlying structure of language. It must therefore be incorporated in all attempts to describe, explain and understand language.

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A Functional Explanation for an Unexpected Case of Ambiguity (S or ~S)*

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Ambiguity occurs regularly in all languages. However, it is rare for a construction to be ambiguous with two opposite readings, namely, R and the negation of R. The rarity of this type of ambiguity is, of course, not surprising, since it is potentially disruptive to the communicative function of language. What is interesting to the linguist is the question: how can a construction with such an unexpected type of ambiguity function in speech? One does not expect a general situation in which after the speaker has uttered a sentence, X, the hearer has to decide whether X means, for instance, the affirmative, "I like you", or the negative, "I dislike you". That would constitute a serious obstacle to human communication. Hence, one may suspect that if and when such construction is found, the discourse context in which the construction means the affirmative might be different from the discourse context in which the construction means the negation of R. Such a suspicion turns out to be correct, as I will demonstrate in this paper. The case of unexpected ambiguity considered in this paper is taken from Mandarin Chinese.[1] I will attempt to show that

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* I am grateful to Dwight Bolinger, H.F. Huang and Sandra Thompson for their helpful criticism and suggestions. I am also grateful to Larry Hyman for presenting an earlier version of this paper in my absence at the Fifth Annual Meeting of the California Linguistics Association and relaying to me the valuable comments of the participants, including himself, at the Meeting. The preparation and research of this paper was supported by a Fellowship from the American Council of Learned Societies.

[1] Larry Horn pointed out at the CLA Meeting that a parallel case of ambiguity exists in English. Consider the sentence

(a) I miss not being in Berkeley.

which has two readings. (i) I miss being in Berkeley; (ii) I miss being away from Berkeley. A detailed analysis of (a) is beyond the scope of this paper. Suffice to note that the ambiguity can be traced again to the problems of scope and discourse presupposition, two major factors in the Chinese case of ambiguity presented in this paper. In reading (i) but not in reading (ii), the crucial explanation lies in discourse presupposition and the functional properties of the construction in question.

The ambiguous construction involves the expression, *chā-yīdiar*, 'miss-a-little'. An example of this ambiguous construction is sentence (1).[2]

(1) Tā chā-yīdiar méi zoŭ
he miss-a-little not leave
(a) *Expected reading*
He barely left.
(b) *Unexpected reading*
He almost left.

On the other hand, sentence (2)

(2) Tā chā-yīdiar zoŭ le
he miss-a-little leave aspect
He almost left.

which also contains the expression, *chā-yīdiar*, is not ambiguous. Contrasting sentence (1) with sentence (2), we observe the following facts: first, sentence (1) is ambiguous but sentence (2) is not; second, (1) contains the negative verb phrase, *'not leave'*, whereas (2) contains the affirmative VP, '*leave*'; thirdly, the second reading of (1) communicates the same message as does sentence (2); and finally, both sentences (1) and (2) contain the expression *chā-yīdiar*, 'miss-a-little'. On the basis of these facts, two questions can be posed. The first question is: how can sentence (1) have two contradictory interpretations, one of which communicates the same mes-

the negative NOT is excluded from the scope of MISS. Other similar but not parallel cases are found in the synonymity of the following pairs of sentences (noted by Jim Bauman and Mark Okrand)

(b) (i) I could care less
(ii) I couldn't care less
(c) (i) I loosened the screw
(ii) I unloosened the screw

An identical case in Spanish was brought to my attention by Dwight Bolinger:

(d) Por poco me ahogo
by a little me smother

Por poco no me ahogo
by a little NEG me smother
(i) I almost didn't smother
(ii) I almost smothered

According to Bolinger (personal communication), the ambiguity of sentence (e) can be explained on the basis of the same kind of discourse presuppositions as the Chinese case.

[2] For some native speakers of Chinese, a pause after the phrase, *chā-yīdiar*, is preferred in the case of the unexpected reading (b).

sage as sentence (2) does? The second question is: why isn't sentence (2), which also contains the expression, *miss-a-little*, ambiguous?

I will begin my analysis with the first question. In order to answer this question, we need to examine three aspects of sentence (1): the first concerns the nature of the expression, *chā-yīdiar* 'miss-a-little'; the second deals with the structure of the sentence; and the third is the discourse presupposition of sentence (1) and the different discourse contexts in which the (a) reading and the (b) reading of sentence (1) may be obtained.

Consider the expression, '*miss-a-little*'. As its gloss implies, etymologically it is a verb-object construction. *Chā* meaning 'to be short of' still functions as a verb in present-day Mandarin. Sentence (3) shows *chā* as the main verb.

(3) Tā haí *chā* wǔ-kuài-qián
he still short of five dollars
He is still five dollars short.

Structurally, however, the expression, *miss-a-little*, always modifies a verb phrase. It can not exist as an independent VP. Hence sentence (4)

(4) *Tā *chā-yīdiar*[3]
he miss-a-little

is ungrammatical. On the other hand, *miss-a-little*, which always occurs in front of a VP is distributionally unlike any adverb in Mandarin Chinese. For example, most adverbs can be preceded by the negative particle *bū* as shown in sentence (5).

(5) Tā bū chángchang shēng-qì
he not often annoyed
He is not often annoyed.

But, the negative particle, *bū*, cannot precede *miss-a-little* as the ungrammatical sentence (6) shows:

(6) *Tā bū chā-yīdiar shēng-qì
he not miss-a-little annoyed

The most significant distributional difference between *miss-a-little* and regular adverbs is that the former can be fronted, but adverbs typically cannot undergo fronting. For example, we may front the expression, *miss-a-little*, in sentence (1) yielding sentence (7)

(7) Chā-yīdiar, tā méi zoǔ
miss-a-little, he not go

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[3] This construction is acceptable only as an incomplete sentence, e.g. a short, abbreviated answer to a question.

If the adverb, *chángchang* meaning 'often' in sentence (5) is fronted, the result is sentence (8):

(8) *Chángchang tā (bū) shēng-qì
often, he (not) annoyed

which is ungrammatical. I will return to sentence (7) later to discuss its meaning. What I intend to illustrate at this point is the difference between adverbs and the expression, *miss-a-little*.

The above analysis of the expression, *miss-a-little*, suggests that the expression is a verb modifier in the sense that it always precedes a verb and is semantically associated with the verb it precedes. On the other hand, it has its own syntactic properties distinguishing it from regular adverbs in Mandarin. On the basis of our analysis of the expression, *miss-a-little*, we can now infer that the structure of sentence (2), the unambiguous sentence,

(2) Tā chā-yīdiar zoŭ le
he miss-a-little leave aspect
He almost left.

could be roughly represented by Figure (a). Figure (a) makes two related claims. One is that the expression, *miss-a-little*, modifies the verb; the other is that *miss-a-little + verb* form a constituent. Support for the second claim is provided by the evidence present in sentence (9),

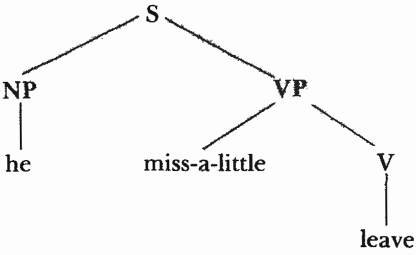

Figure (a)

(9) Chā-yīdiar zoŭ le de nèi-ge-rén . . .
miss-a-little leave aspect relative that-person
clause
marker
The person that almost left . . .

In sentence (9), the embedded relative clause is *miss-a-little + leave*, indicating that the second claim of Figure (a), namely, *miss-a-little + verb* form a constituent, is correct.

Now if Figure (a) correctly represents the structure of sentence (2) and if the difference between sentences (1) and (2), other than the ambiguity of (1), is the presence of the negative particle, *méi*, in sentence (1), it follows that Figure (b) must represent at least one of the structures of sentence (1).

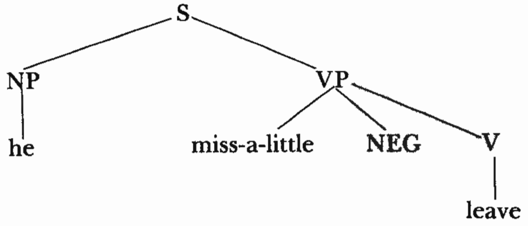

Figure (b)

In Figure (b), *NEG + leave* is in the scope of the modifier, *miss-a-little*, as the verb, *leave*, is in the scope of the same modifier in Figure (a). Semantically this scope relation may best be clarified with the following illustration. Imagine the goal to be attained is '*to leave*'. If *to leave* is in the scope of the expression, *miss-a-little*, we may conceive of the meaning of *miss-a-little + leave* as denoting that the goal, *to leave* is being *missed* by just a *little bit*. Hence, the logical meaning represented by Figure (a) is, *he almost left*, i.e., *he almost reached the goal which is 'to leave'*. In Figure (b), we have *NEG + leave* in the scope of the modifier. In this case, what is being *missed* is the goal, *NOT leave*. If one is just short of *NOT* leaving, the logical meaning is clearly, *one barely left*. Hence, the structure of sentence (1) represented by Figure (b) corresponds to the (a) reading, which is called *the expected reading*. The (a) reading is expected because it is logically congruent with the reading of sentence (2), which differs from sentence (1) only in that it doesn't have the negative particle. In other words, if the logical meaning of (2) represented by Figure (a) is, *he almost left*, it is to be expected that the logical meaning of sentence (1) represented by Figure (b), which is the negative counterpart of Figure (a), should be, *he barely left*.

The expected reading, (a), of sentence (1), as we have seen, is fairly straightforward. Let us now turn to the (b) reading of (1), *the unexpected reading*. The (b) reading is unexpected because it conveys the same message as sentence (2), in spite of the fact that sentence (1) contains the negative particle which is not present in (2). It is obvious that Figure (b) does not account for the unexpected (b) reading of sentence (1). In order to understand the structure of sentence (1) that yields this unexpected reading, we will examine three pieces of evidence.

First, if the expression, *miss-a-little*, is fronted, yielding sentence (7),

(7) Chā-yīdiar, tā méi zoǔ
miss-a-little, he not leave

the ambiguity of sentence (1) is lost. Sentence (7) can only have the unexpected reading (b), *he almost left*.

The second piece of evidence is that a pause may occur between *miss-a-little* and *NEG* + *leave* in sentence (1). When this pause is inserted, the meaning of (1) is again exclusively the unexpected reading (b), *he almost left*.

The third piece of evidence concerns the discourse presupposition. The discourse presupposition associated with a sentence is that of the discourse leading up to the sentence at issue, not that of the sentence itself. In other words, the discourse presupposition represents a proposition that has already been aired by the time the sentence in question is uttered. It is a proposition that is very much at the forefront of the mental frames of the participants in the discourse. Givón (1975) has pointed out that the context in which a negative sentence can occur must render the corresponding affirmative likely. To illustrate this point, we observe that it is natural to initiate a discourse with the affirmative sentence:

(10) There are monkeys in Taiwan.

But it is very odd to begin a conversation with the negative statement:

(11) There aren't any monkeys in Taiwan.

However, sentence (11) becomes perfectly natural in a setting in which its corresponding affirmative has already been stated or made known. In other words, the discourse presupposition of a negative sentence is never neutral. It requires that the corresponding affirmative statement either be uttered earlier in the discourse, or simply be assumed or inferred through extra-linguistic factors by the participants in the discourse. In the case of sentence (1), we observe that it contains a negative verb phrase. Thus, the corresponding affirmative, *he left*, must be a part of the discourse presupposition by the time sentence (1) is uttered. This is a crucial factor that will lead us to the structure of sentence (1) which yields the unexpected reading, (b), *he almost left*. Recall the first two pieces of evidence: if a pause is inserted between *miss-a-little* and the rest of the sentence, the result is an unambiguous sentence with the unexpected reading (b). Similarly, if *miss-a-little* is fronted, resulting in sentence (7), again the meaning is exclusively the unexpected reading, (b). These two pieces of evidence suggest that when sentence (1) yields the unexpected reading, the expression, *miss-a-little* is independent of the verb phrase, NOT *leave*. The expression seems to be in coordinate relation with the VP, NOT *leave*. On the other hand, it was pointed out earlier in this paper that the expression, *miss-a-little*, always modifies a verb and cannot occur independently. Hence, we are left with the explanation that when the sentence (1) yields the unexpected reading, (b), the verb modified by the expression, *miss-a-little*, is deleted, and is not present in the sentence itself. What is deleted, then, must be the verb, *leave*, which is a repetition of the discourse presupposition, *he left*. In other words, the discourse presupposition of the negative statement, *NOT* + *leave*, makes it possible for an ellipsis to occur in sentence (1) so that the unexpected reading can be obtained. We have, therefore, arrived at Figure (c) which represents the structure of sentence (1) denoting the unexpected reading, (b), *he almost left*. To paraphrase the meaning of the structure represented in Figure (c), we may describe the unexpected reading (b) of sentence (1) as: "He missed just by a little bit the goal of leaving and thus, he has not left".

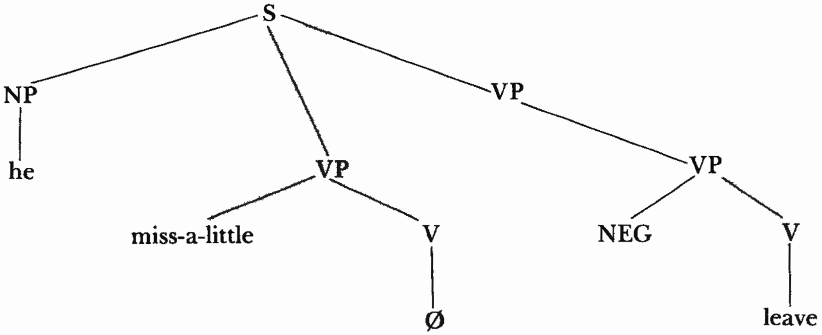

Figure (c)

Since we have, at this juncture, elucidated the structures of sentence (1) for both readings, we should consider the question: how do the circumstances in which one of the two readings of sentence (1) occurs differ from those in which the other reading occurs? In order to facilitate our discussion of the type of sentences represented by sentence (1), we will refer to the linear sequence (12):

(12) NP_1 + miss-a-little + NEG + V_1

If the expected reading (a) is to be realized, the discourse context must render the event represented by (13)

(13) NP_1 + V_1

highly desirable. The discourse context (14) provides an appropriate circumstance.

(14) John: xìngkui tā zoŭ le
fortunate he leave aspect
Fortunately he left.
Carol: zhēn weíxiăn! Tā chā-yīdiar meí zoŭ
real dangerous! he miss-a-little not leave
It was really dangerous! He barely left.

In the context of (14), sentence (1) as it occurs in Carol's speech only conveys the expected reading (a) because the context establishes that the event, *he left*, is desirable.

Conversely, if the unexpected reading (b) is to be realized from sentence (1), the discourse context must render the event represented by (13) highly undesirable. (15) provides such a context:

(15) Peter: yàoshi tā zoŭ le, kĕ j̀iu zaōgao le
if he leave aspect, indeed messy aspect
If he has left, it will be a real mess.
Mary: wŏmen yùnqi haí haŏ. Tā chā-yīdiar meí zoŭ
we luck still good. he miss-a-little not leave
We are still lucky. He almost did leave.

In the context of (15), sentence (1) as part of Mary's dialogue only conveys the unexpected reading because the context establishes the event, *he left*, as highly undesirable.

In general, if we take an event that is undesirable under the normal circumstances, the structure (12) will yield the unexpected reading. For example, consider the event represented by sentence (16):

(16) wŏ de māma sĭ le
I genitive mother die aspect
particle
My mother died.

The message of (16) is clearly undesirable under normal circumstances. Hence sentence (17)

(17) wŏ de māma chā-yīdiar meí sĭ
I genitive mother miss-a-little not die
particle

will under normal circumstances be given the unexpected reading, *my mother almost died*, by a native speaker without being provided a discourse context.

On the other hand, if we have an event that is highly desirable under normal circumstances, a sentence represented by the structure (12) in isolation will elicit the expected reading from a native speaker as a general rule. Consider, for example, the following sentence:

(18) wǒ chā-yīdiar meí jígé
I miss-a-little not pass

If a native speaker is asked to interpret sentence (18) without a discourse context, he will probably give the expected reading, *I barely passed*, since '*I pass an examination*' normally represents a highly desirable event.

The analysis provided so far has answered the first question posed at the beginning of this paper: how can sentence (1) have two contradictory interpretations, one of which conveys the same message as in sentence (2)? I will now attempt to answer the second question: why isn't sentence (2), which also contains the expression, *miss-a-little*, ambiguous?

Let us recall that one difference between sentences (1) and (2) is that (1) contains a negative VP but (2) does not. In our discussion of the discourse presupposition, we pointed out that the context in which a negative sentence occurs must render the corresponding affirmative likely, but the affirmative sentence can be used in discourse contexts which are presupposition neutral. In other words, sentence (2)

(2) Tā chā-yīdiar zoǔ le
he miss-a-little leave aspect
He almost left.

does not require that either the proposition, *he left*, or the proposition, *he didn't leave*, be assumed by the participants of the discourse prior to its utterance. Such a situation does not leave any possibility for the expression *miss-a-little* to modify a deleted VP because there is no consistent discourse presupposition that can render such a deleted VP recoverable semantically. Thus, the type of ambiguity that is exhibited by sentence (1) cannot occur in sentence (2).

In conclusion, I have presented an unexpected case of ambiguity involving two contradictory readings and offered an analysis of the construction explaining the WHY and the HOW questions concerning the ambiguity. The theoretical lesson to be drawn from the facts and the analysis presented in this paper is that the explanation of certain linguistic data is not to be sought solely in their structural properties, but also in the pragmatic context in which they perform a communicative function.

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Will the Real Direct Object in Bitransitive Sentences Please Stand Up?

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The cross-linguistic and experimental research to be reported in this paper had its origins in theorizing by the first author about relations between cognizing and sentencing — a preliminary to what might be called an Abstract Performance Grammar (cf. Watt 1970). He was ably abetted in these explorations by the second author. One of the basic notions was that the cognitive structures which underlie sentence comprehending and producing have their origins in *pre-linguistic* processing of information in perceptuo-motor channels, this input involving perceived entities and their participation in perceived action and stative relations. Given both the nature of the physical world with its space/time coordinates and the nature of human organisms generally, it seemed likely that the most basic simple cognitions would be Actor/Instrument – Action – Recipient/Object for action relations and Figure – State – Ground for stative relations. Even the not-so-astute reader will recognize these simple cognitions as characterizing SVO languages.

But what about pre-linguistic events which involve *three* perceived entities participating in a single relation? One such case (of many) is that of transfer of possession, in which one animate entity is perceived transferring a (usually) inanimate entity to another animate entity, e.g., BROTHER GIVES BOOK SISTER.[2] Is this to be conceived as a simple or a complex cognition? Putting on the booties of pre-linguistic childhood, it seemed intuitively clear that BROTHER is the Actor, GIVE and BOOK are part of the Action relation, and SISTER is the Recipient. Yet in traditional grammars for sentencing — where, for SVO languages, we can have either *Brother*

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[1] Christine Tanz is now in the Department of Psychology, University of Arizona, Tucson.

[2] We use ALL CAPS to distinguish nonlinguistic, perceptuo-motor cognizings from linguistic sentencings, the latter being expressed in the usual italics.

gave a book to Sister or, alternatively, *Brother gave Sister a book* — *book* is called the Direct Object and *sister* is called the Indirect Object.[3] Our booties-based intuitions seemed to be leading to a radical hypothesis — that the "real" DO in bitransitive sentences is what has traditionally been called the IO.

Two major avenues for testing this hypothesis suggested themselves: one, cross-language linguistic evidence for a more intimate relation between V and DO than between V and IO; the other, experimental psycholinguistic evidence for a tighter cognitive "binding" between V and DO than between V and IO in sentence processing and recall. The latter we could handle ourselves (at least, for American English), but we would need help with the former. C.E.O. had been a friend of Joe Greenberg's since the summer of 1953 (when we both were participants in the SSRC-sponsored summer seminar on psycholinguistics (cf., Osgood and Sebeok 1954), he was familiar with Greenberg's writings on language universals (e.g., 1963, 1966), and he knew about Greenberg's centrality to the work of the Stanford Language Universals Project. So, when a trip to the West Coast materialized in the spring of 1973, Greenberg was on the agenda. He was interested in the hypothesis (without committing himself one way or the other!) and suggested several directions cross-linguistic investigations might take. The up-shot was an arrangement whereby one of his (then) students, Philip Sedlak, would be employed by our Institute of Communications Research for the following summer to follow up on these (and other) directions; the research continued quite a bit longer under Sedlak's own motivation, and a report has recently appeared (1975).

Theoretical background

In his 1963 paper on "Some Universals of Grammar", Greenberg found it "convenient" to set up a tripartite word-order typology of languages, based on the dominant order for simple, active, affirmative, declarative (SAAD) sentences. That such a tripartite system, involving Subject (S), Verb (V) and Object (O), was something more than "convenient" is evident from the fact that many of the 45 universals (often of an implicational sort and often in fact statistical rather than without known exceptions), he derives from his sample of 30 languages would be obscured if the familiar bipartite (NP + VP) system were employed. For example, "in declarative sentences with nominal subject and object, the dominant order is almost always one in which the subject precedes the object" (1963:61); thus three of the logically possible orderings, VOS, OSV, and OVS, are "excessively rare" — in fact, only a few VOS having been reported, never to our knowledge any of the OSV or OVS types. Of the three viable dominant types, SVO, SOV, and VSO, it might be noted that the last would interpose S between the V and O predicate (VP) components.[4]

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[3] It is interesting that one ethnolinguist (Cole 1955), in his *An Introduction to Tswana Grammar*, expresses the same intuition about the IO being the 'real' DO by using the phrase "principal object" to refer to the former; no doubt there are many other instances of which we are unaware.

Evidence for the basicness of the SVO structure

Our intuition about the nature of simple cognitions is, as noted above, that they have an SVO structure. Is there any support for this notion in the frequency distributions of the three viable dominant types? Greenberg's (1963) sample of 30 languages has 43% typed as SVO, 37% as SOV, and 20% as VSO; Sedlak's (1975) much later sample of 45 languages types 60% as SVO, 31% as SOV, and only 9% as VSO; Blansitt (1973) worked with a much larger language sample, 106 languages, and classifies them as 58% SVO, 30% SOV, and 12% VSO.[5] A rough averaging of these estimates, giving more weight to the larger samples, would be about 55% SVO, 35% SOV, and 10% VSO. It would be interesting to have diachronic data on shifts in dominant types, but little seems to be available; Greenberg (1963:63) does give as his Universal #6 that "all languages with dominant VSO order have SVO as an alternative or as the only alternative basic order". Consistent with our intuitions, SVO is clearly the dominant type — and if one looks at the numbers of humans using languages of the various types, one finds that with very few exceptions (e.g., TURKISH, KANNADA, PUNJABI — all SOV) the "major" world languages are SVO.

But if the "natural" mode of pre-linguistic cognizing for all humans is, indeed, SVO, how then does one explain the existence of languages with dominant SOV and VSO orders? When one considers *statistical* universals, it must be kept in mind that ". . . we always have a number of tendencies at work at the same time"[6] and these competing factors are likely to produce counter-examples (witness, above, that only VOS types, which bring the predicate back together, appear to go against the S before O law). It is also the case that where alternative constructions (transformations on the presumed basic order) are available in a language, social factors may render the "unnatural" construction preferable stylistically (e.g., the preference for the passive OVS in much depersonalized academic writing).

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[4] However, in a letter to C.E.O. (6/21/74), Greenberg notes that the occurrence of VOS, but apparently not any other O-before-S types, may itself reflect a competing tendency to "restore the integrity of the predicate" by a shift to the *VOS* from the *VSO* basic ordering.

[5] It may well be that Greenberg's original sample of 30 languages was deliberately nonrandom, amplifying the less frequent SOV and VSO as against the more frequent SVO, in order to provide more balanced evidence in his search for grammatical universals.

[6] Greenberg, 6/21/74 letter to C.E.O.

One way around this problem would be to demonstrate that, regardless of dominant-order type, *in the process of language development in children there is initially a relatively fixed SVO ordering in "sentence" productions*. Finnish is an SVO-type language basically, but, since it is highly inflected, other orders are frequent in adult language; Bowerman (1973) challenges the above proposition, reporting that Finnish children use variable word orders. Serbo-Croatian is another SVO language with highly variable adult word ordering permitted by its being heavily inflected also. In a recent study — intensive over time with two Dubrovnik children and checked cross-sectionally with 40 other children — Radulović (1975:Abstract p. 1) concludes, consistent with our proposition, that ". . . contrary to the adult model . . . children's spontaneous utterances are found to follow a fairly inflexible word-order pattern of subject-verb-object." She also checked the sentences used by the mothers of the two children in their interactions; 88 different orders in a sample of 100 sentences were used by one mother and 87/100 by the other. The inflectional system, interestingly enough, first gets established in the SVO order and only then does freedom in word-ordering begin to appear. What we need, of course, is evidence on SOV- and VSO-type languages. Although German is clearly SVO by Greenberg's (1963) criterion (order in the SAAD-type sentence), Roeper (1973) suggests that the "underlying order" may be SOV — on the ground that embedded sentences in German take that order (e.g., *Hans, who Wurst eats, loves Maria*) — and he reports that in *imitation* of complex sentences with SOV embeddings most of the 45 (four-and five-year-old) children studied kept the SOV order given (which could be just parroting). But on a *comprehension* task, most children followed the SVO model. It would be strange, indeed, if children picked up an SOV order from *embedded* adult sentences — unless Germans were somehow innately SOVs!

Outline of a general theory of cognizing and sentencing

The first author's theory of cognizing and sentencing involves three levels of processing on both sensory (comprehending) and motor (expressing) sides. The *Projection Level* (linking peripheral inputs and outputs to sensory and motor projection areas of the cortex) and the more central *Integration Level* (in which sensory "forms", in the gestalt sense, and motor "programs" are integrated, reflecting redundancies in perceiving and behaving) have been discussed in detail elsewhere (see the Discussion section of Osgood and Hoosain, 1974, for a recent presentation). The main relevance here is that in the total cognizing process — whether via nonlinguistic, perceptuo-motor channels (comprehending perceived events and behaving appropriately) or via linguistic channels (comprehending and producing sentences) — sensory integrations (meaningless forms) are the *input* to the *Representational* (meaningful) *Level* and motor integrations (cortical programs for overt behaving, verbally or otherwise) are the *output* from this level.

At least the following "mechanisms" (functional types of information processing by as yet unspecified means in the CNS) must be postulated at this highest (most central) Representational Level: a LEXICON, an OPERATOR, a BUFFER, and a long-term MEMORY — only the first two of which will be of prime concern here. The LEXICON is a semantic encoding and decoding mechanism — a "process" rather than a "storage" type of memory, having the advantage of being re-usable at a very fast clip. The OPERATOR is the mechanism that gives structure to related sets of outputs from the LEXICON and within which the dynamic semantic interactions among such sets occur. Inputs to the OPERATOR may be from either the LEXICON (in which case we speak of "cognizing") or the MEMORY (in which case we speak of "re-cognizing"); outputs from the OPERATOR may be either to the MEMORY (in comprehending) or to the LEXICON (in expressing). When the sets of semantic input to or output from the LEXICON do not correspond to the "natural" ordering in terms of which the OPERATOR is structured, temporally misordered sets will be briefly stored in the BUFFER. In effect, the ordering of semantic information in the OPERATOR in interaction with the BUFFER constitutes the transformational aspect of this model.

The use of the terms *encoding* (for the inputing of information to the Representational Level via the LEXICON) and *decoding* (for the outputing of information to the motor expressive systems via the LEXICON) seems entirely *apropos* here: nonmeaningful, analogically represented perceptual forms are *en*-coded into *meaningful*, digitally represented semantic-feature sets on the input side, and the same semantic-feature sets are *de*-coded into nonmeaningful motor programs on the output side. It should be emphasized that everything that transpires at the Representational Level is based upon transfers of and operations on the semantic code-strips outputed "upwards" from the LEXICON; thus, the "semantic component" has been fully substituted for the "deep syntactic component" of the standard generative grammar (Chomsky 1965) of linguistics — as a step toward an abstract *performance* grammar.

The most critical mechanism for present purposes is the OPERATOR. A *simple cognition* is defined as the representation in the OPERATOR of a perceptual event or linguistic sequence involving a single action or stative relation between two perceived entities (perceptual) or noun phrases (linguistic). It is thus assumed that simple cognitions are *tripartite* in structure, the three components being M_1 (the meaning of one perceived entity, later of a subject NP), – – (M) – –> (the meaning of the perceived action or stative relation, later of a verb phrase), and M_2 (the meaning of another perceived entity, later of an object NP). It is further assumed that all *complex cognitions* are analyzable into conjoined sets of simple cognitions — indeed, *must* be so analyzed in order to be comprehended (and, for economy in expression, simplexes may be synthesized into complexes in sentence production, of course).

The Naturalness Principle

The Naturalness Principle in sentencing is simply that the more surface structures correspond in their ordering of semantic information to that characteristic of pre-linguistic, perceptual cognizing, the more easily (other things being equal, of course) will they be understood and produced. Since pre-linguistic cognizing is prior to linguistic sentencing (in both species and individual development), it seems reasonable that the already available structures of the former would be "taken over" by the latter. The basic structure of a simple cognition is assumed to be [M_1 – – (M) – – > M_2], but what determines the *semantic content* of this cognitive structure?

Again putting on the "booties" of early childhood, we note the following: not only does one usually perceive Actors before Recipients, with the Actions more typically associated with the former than the latter (e.g., THE BIRD FLIES ONTO THE BRANCH), but the $^+$Animate (often $^+$Human) and $^+$Movement features characteristic of Actors presumably have more motivational salience than the $^-$Animate and/or $^-$Movement features often characteristic of Recipients (indeed, there is hardly any way to express THE BRANCH RECEIVES (?) THE BIRD). There is also much gestalt-type evidence that Figures somehow "stand out" from Grounds (THE BIRDS ARE IN THE BUSH is somehow "natural" while THE BUSH IS AROUND THE BIRDS seems most "unnatural"); and again note that Figures are more likely to have $^+$Animate and/or (at least, potentially) $^+$Movement features than Grounds (hence the "naturalness" of THE BALL IS ON THE ROCK and the "unnaturalness" of THE ROCK IS UNDER THE BALL and note that THE BALL IS UNDER THE GLASS is somehow a more likely description than THE GLASS IS OVER THE BALL). So we conclude that the natural order of action relations will be Actor – Action – Recipient in simple cognitions and the natural order of stative relations will be Figure – State – Ground.[7]

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[7] Although we consider Naturalness to be the most important ordering principle, being based not only on regularities in pre-linguistic experience but also upon a whole succeeding

Simplexes and complexes

Simple cognitions, symbolized as $[M_1 - - (M) - - > M_2]$ above, are often conjoined into complex cognitions of varying number and complexity. Thus

> (At Devonshire School) boys and girls create and perform in ballets and plays

actually, as freshly cognized, comprises eight conjoined cognitions,

> [boys create ballets] and [boys create plays] and [boys perform in ballets] ... and [girls perform in plays]

which must be analyzed or synthesized in the OPERATOR in order to be comprehended or produced, respectively. On the other hand,

> (At Devonshire School) boys create ballets and girls perform in plays

involves only two conjoined cognitions,

> [boys create ballets] and [girls perform in plays]

even though the lengths of the sentences and the numbers of lexical words are approximately the same. The performance theory being developed here clearly must predict that 8-cog sentences will be more difficult to process than 2-cog sentences.[8] Using 2-cog, 4-cog, and 8-cog versions of 12 sentences of the type shown above, and given the initial (parenthetical above) phrases as cues, Hoosain and Osgood (in preparation) have found that this prediction holds for recall of the 12 sentences presented as a set on cards — whether measured by number of lexical items correctly recalled, by number of complete cogs correct, or by number of perfect recalls. This held for both American English and Persian (Iranian Farsi), the latter being an SOV-type language.

Another prediction from the theory would be that increasing the complexity of the *constituents* of simple cognitions, e.g.,

M_1 $- - (M) - - >$
[the three little old men / were waiting anxiously for /
M_2
the king's judgment],

will create less processing difficulty than increasing the number of conjoined cognitions, as above, with sentence lengths and numbers of lexical

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life-time of perceptual cognizing in parallel with linguistic sentencing, there are other ordering principles based upon salience to the speaker (e.g., vividness of components semantically and salience attributed motivationally by the speaker), and these may be either congruent or incongruent with the Naturalness principle — see Osgood and Bock (1976) for details.

[8] Throughout this paper we will often use "cog" as shorthand for "cognition".

items held constant.[9] This little experiment remains to be done. However, Forster and Olbrai (1973) report relevant data (albeit, in a study designed for other purposes). The processing times for single-clause (simplex) and two-clause (complex) sentences were compared, the constituent complexity of the former being increased to match the latter in length and number of lexical items. Illustrative of the sentences used are: (simplexes) *The wealthy child attended a private school, John smoked cigars throughout the dreary play*; (complexes) *The dress that Pam wore looked ugly, The choir sang hymns while we prayed.* Semantically anomalous and ungrammatical sentences were also presented. Processing times (between sentence-slide onset and the subject's judgment of grammaticality) for simplexes (single-clause) were significantly faster than those for complexes (double-clause). It is also noteworthy that plausible sentences, in terms of knowledge of the world (like *The choir sang hymns while we prayed* above), were processed faster than implausible sentences (like *The babies rang bells while he fired*).

The relevance of these experiments on sentential complexity here is this: if increasing the number of clauses in a surface structure, and hence the *number of cogs* causes more processing difficulty than increasing the *complexity of constituents* within a single clause, then languages should tend to develop means of minimizing the former at the expense of the latter — that is, develop means of packing more information into simplexes.

Three entity cognitions

Cognizings and sentencings that involve three entities (or NPs) but only one relation (or VP) provide a case in point and put pressure on a theory that postulates an underlying tripartite structure for all simple cognitions. The meaning of the third entity (or NP) must either be 'absorbed' in one of the three cognitive components, thereby preserving a simple cognition, *or* it must be demonstrable that the cognition is in fact a complex, analyzable into two simplexes.

The clearest case for the latter type of resolution is where two *relations* are conjoined and only two entities are involved, e.g., *Annabelle stuffed and* (then) *roasted the turkey*, which would be represented as

> [Annabelle stuffed the turkey] and (then) [Annabelle roasted the turkey],

with the only exceptions to the "rule" seemingly being cases where the conjoined relations are redundantly near-simultaneous and form a trite phrase, such as *hopped and skipped* (down the walk), or *chewed and swallowed* (his food). Similarly, conjoined subjects or predicates that are clearly unrelated, e.g., *my wife and ex-President Nixon's secretary are blindly loyal* as subject NPs or *Tom is stupid but honest* as predications, must be treated as complex cognitions, whereas clearly *related* (or trite) conjoined subjects or predicates will typically be processed as single cognitions, e.g., *Dick and Diana play a mean game of bridge* (where the two are, say, husband and wife) or *Tom is just fine and dandy*. One suspects that, psycholinguistically speaking, there is a probabilistic continuum in such cases between cognizing as simplexes or as complexes.

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[9] This assumes that the complex NPs are already familiar as wholes and are therefore being re-cognized; novel NPs, e.g., with relative clauses like *the three men who were little and old* or like *the judgment that the king was to make*, are themselves complexes cognitively.

Before going further, it should be pointed out that in this model the relations (-- (M) -- >) in simple cognitions are what, in the mathematical theory of graphs, are termed signed and directed *relations*, the 'signing' being associative or dissociative (*is* vs. *is not* in the simplest case) and the 'directions' being Actor/Instrument-*to*-Recipient/Object (action relations) or Figure-*to*-Ground (stative relations). It should also be noted that — except probably for the verb *to be* — relations have much richer semantic coding (meaning) than mere sign and direction, as is implied by the parenthetical (M) above. Additional features serve to distinguish among, e.g., having, causing, giving, etc., relations and, of course, the even finer less frequent distinctions among complex verbs like *roll* vs. *run*, *hate* vs. *despise*, *manipulate* vs. *protect*, and so on.

Prepositional third entities. A large and varied class of three-entity cognitions is those in which a third entity provides a kind of context, delimitation, specification, etc., for the action (DADDY PUT MY BALL IN HIS POCKET) or state (THE CAT IS ON THE PILLOW BY THE WINDOW). In such cases, linguistically speaking, the verb has three 'arguments' (cf., for example, Fillmore 1971; Bierwisch 1971), and one problem for this theory is which of the two nonsubject NPs functions as the M_2 of simple cognitions (BALL OR POCKET, PILLOW OR WINDOW). The same question holds for sentences, of course — for example, in *Khrushchev banged his shoe on the table*, or even more clearly in *Mack put the knife on the table*. Another problem for a theory that postulates [M_1 -- (M) -- > M_2] or S–V–O, as the 'natural' structure of simple cognitions is to show how the third entity (NP) can be 'absorbed' either into

[DADDY / PUT IN HIS POCKET / MY BALL]
[Khrushchev / banged his shoe on / the table],

or into one of the other two entities, e.g.,

[DADDY / PUT / IN-POCKET-MY-BALL]
[Khrushchev-his-shoe / banged on / the table],

or to show convincingly that the three-entity cognition in question is, indeed, a complex (2-cog) cognition that has undergone redundancy deletion.

The last resolution would seem to hold for stative relations like THE CAT IS ON THE PILLOW BY THE WINDOW, which would break down cognitively into

[THE CAT / IS / ON THE PILLOW] and [THE PILLOW / IS /) BY THE WINDOW]

with the reversed sequencing of cogs holding if the situation is perceived as THE CAT IS BY THE WINDOW ON THE PILLOW. Action relations do not as readily break into such dual-cogs-with-deletion,[10] and of the two 'absorption' alternatives above — into the VP vs. into another NP — the former seems intuitively much more natural, at least for English. Our general proposal — for action prepositional cognitions as well as for bitransitive cognitions — is that, by virtue of its potential expansion in semantic complexity, the relation (VP) is the most likely component of the tripartite simple cognition to 'absorb' the semantic information in the third entity or NP.[11] However, applying three criteria (and there are undoubtedly others), it appears that cognizing prepositional three-entity inputs as expansions of the relations (VPs) of simplexes or as double-cog complexes (with redundancy deletion) is again a continuous rather than a nearly discrete function.

The first criterion used was (1) *substitutability of a simple verb for the verb + prepositional phrase*, the reasoning being that, if this can be done acceptably, then 'absorption' of the third NP into the relation is supported. Using the example above, English provides a ready substitute for *put in his pocket*, yielding *Daddy pocketed my ball*, but there is nothing that would yield *Daddy my-balled his pocket*, implying further that *my ball* is the 'real' M2. There are many such lexical substitutions in English — *the man brought the boat onto the beach/the man beached the boat*; *the secretary put the folder in the file/the secretary filed the folder*; *Johnny took the leash off the dog/Johnny unleashed the dog*, and so on. But there are many lexical gaps, as far as such substitutions are concerned; English does not provide any single verb for *Khrushchev shoe-banged the table*, nor for *the man saucered the cup*, nor *the man keyed the lock* (for *the man put the key in the lock*), and so on. Do such counter-examples weaken the hypothesis or merely testify to the fact that there are many lexical gaps in the multidimensional semantic space of English — which easily could be filled and probably are in certain other languages?

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[10] However, see the Discussion section for consideration of the possible development of prepositions in bitransitive sentences from serial-verb constructions in languages.

[11] And this notion, of course, implies that, cross-linguistically, verb incorporation of nouns should be more frequent than noun incorporation of nouns (the latter to be distinguished from nominal compounding, where one of the nouns becomes adjectival in function, e.g., *doll house*). We are not aware of any data on this, although it should be available in the Stanford Universals Project.

Donning the booties of childhood again, a second criterion was that (2) *reversing the order of the two nonsubject NPs so as to bring that NP intuited as part of the action closer to the verb should yield more acceptable sentences than a shift in the other direction*. Thus *Khrushchev banged his shoe on the table* (where one easily imagines the shoe in hand being a part of the action) seems a more natural description than *Khrushchev banged on the table his shoe*; similarly for *Mack put the knife on the table* vs. *Mack put on the table the knife* (where it is the knife which is moving with the arm) and for *the dancer put the flowers on the trunk* vs. *the dancer put on the trunk the flowers* (the flowers being moved). But there are also many exceptions: *the policeman chased the dog across the street* is clearly more acceptable than *the policeman chased across the street the dog* (even though *chased across the street* clearly seems to be part of the action rather than a locative, as would be the case in *Pierre likes the dog across the street*); similarly, although *the students sprayed paint on the ceiling* is preferable to *the students sprayed on the ceiling paint* (fitting our perception-based expectations), *the students sprayed the ceiling with paint* is definitely preferable to *the students sprayed with paint the ceiling* (counter to expectations).

A third criterion we tried was (3) *passivization*, the reasoning here being as follows: if the two nonsubject NPs can be separated in the transformation, then the prepositional phrase can be considered part of the VP (thus *some mustard was put on the shelf by the cook* but not **some mustard on the shelf was put by the cook*); if the two nonsubject NPs must be shifted intact, then the prepositional phrase is clearly not part of the VP (thus *a boat with a cabin was bought by the professor* but not **a boat was bought with a cabin by the professor*). Note that by this criterion the ambiguousness of the students-painting-the-ceiling example above is resolved in favor of VP absorption — both *paint was sprayed on the ceiling by the students* and *the ceiling was sprayed with paint by the students* are acceptable, but the other passive forms, *paint on the ceiling was sprayed by the students* and *the ceiling with paint was sprayed by the students*, are not. There are some cases where the two forms of the passive are equally acceptable, but have different meanings; an example is *the old lady across the street was helped by the gardener* vs. *the old lady was helped across the street by the gardener*. The passivization criterion seems to divide prepositional three-entity cognitions rather neatly into VP-absorption types and VP-nonabsorption types, and the former generally fit one's intuitions about the prepositional phrase being a part of the action. We are presently analyzing experimental data to see if, in recalling prepositional sentences, there is a tighter 'binding' of the prepositional phrases with the verbs for the former type as against the latter.

In applying these several criteria it was noted that semantics — or, perhaps better, knowledge of the world — plays an important role in either facilitating or blocking phrase reversals and passivizations. Note as an extreme example of this that *the detective chased the Martian with black spots* (as contrasted with . . . *with a stick*) is nonreversible simply because we know that you cannot chase anything by using 'black spots', and the passivization has to be *the Martian with black spots was chased by the detective*. The same would hold for an earlier example, *the professor bought a boat with a cabin* — because we know you cannot buy things using 'cabins' for currency. But note also that such semantic, or knowledge-of-the-world, factors produce 'squishiness' in applying even the passivization criterion: *John fixed the table in the kitchen* transforms naturally into *the table in the kitchen was fixed by John* and only awkwardly into *the table was fixed in the kitchen by John*, whereas, with *wristwatch* substituted for *table*, the reverse passivizations are natural (*the wristwatch was fixed in the kitchen by John*) and awkward (*the wristwatch in the kitchen was fixed by John*) —simply because we have kitchen tables but not kitchen wristwatches!

Bitransitive third entities. In what has been called the "prototype" bitransitive (Blansitt 1973:2-3) or the bitransitive "par excellence" (Sedlak 1975:123-124), a $^{+}$Human Actor voluntarily transfers a $^{-}$Animate Object to another $^{+}$Human Recipient, and the prototypical transfer verb is *give*. Blansitt points out that other basic (that is, voluntary) transfer verbs, like *lend*, *send*, *bring*, *take*, *sell* and *rent*, are more complex semantically (*lend* denoting temporariness, *send* denoting mediateness, *bring* and *take* specifying directions of locomotion, and *sell* and *rent* indicating semantically permanent or temporary transfers for remuneration). Thus, underlying the prototype bitransitive are the notions of personal possession and personal property — and these notions characteristically (if not exclusively) apply to humans and never to inanimate entities. Since the prototype bitransitive does involve an action relation of transferring, and the so-called Direct Object in prototypical *give* cases is the thing being transferred, the intuition that DO will be perceived as part of the action in pre-linguistic experience is much stronger for bitransitives than was the case for prepositional three-entity cognitions — i.e., that

M_1 -- (M) -- > M_2
[PERSON A / TRANSFERS OBJECT X TO / PERSON B]

will be the natural, or basic cognitive structure, with

M_1 -- M_2 -- (M) -- >
[PERSON A / TRANSFERS TO / PERSON B / OBJECT X]

being a transformation which, as can be seen, 'breaks up' the action relation.

In analyzing prepositional three-entity sentences we noted that semantic, or knowledge-of-the-world, factors play a significant role. This, again, is even clearer for bitransitives. In most cases (including the prototypical ones of course) the DO is $^-$Animate and the IO is at least $^+$Animate even if not necessarily $^+$Human (as in *the boy gave a bone to the dog*). This means that most bitransitives can be cognized successfully using only this semantic distinction between DO and IO; no marking or even ordering rules (beyond an S-before-IO rule) should be necessary. This suggests that the marking and ordering rules that are in fact found for bitransitives in most languages are compelled by *non*prototypical cases — the cases where the DO is $^+$Animate (as in *the boy gave the kitten to his sister*) or even $^+$Human (as in *the man brought his daughter to the priest*).

Apparently, according to Blansitt (1973:4), ". . . there is no way to identify corresponding clause types across language boundaries except through *the semantic characterization* of prototype clauses; . . . thus, once the structure or structures of a prototype clause . . . are identified, other clauses having the same structure will be formally specified as members of the same grammatical type" (italics added). And to this Sedlak adds the warning that "syntactic 'sames' may be the surface expression of underlying semantic 'differents' " (1975:123), for example, *Tom brought the book to his desk* (locative) vs. *Tom brought the book to his roommate* (bitransitive) — and to which *we* will add the warning that "syntactic 'differents' may be the surface expressions of underlying semantic 'sames' " (which is the case with the two surface forms for bitransitives in English, of course).

Since the surface structure we postulate as basic in the cognizing of bitransitives is identical to that of many prepositional phrases involving *to* (e.g., *Tom brought the book to his roommate / to his desk* example above), it may be profitable to note the similarities and differences between them. The first criterion we employed with PP-phrases was (1) *substitutability of simple verbs for the V+PP-phrases* (where meaning is unchanged): there are many cases in English where this can be done with bitransitives —*John gave a bath to / bathed his son* (but not **John bathed to his son*), *John made a promise to / promised his wife*, (but not **John promised to his wife*); but, as with PP-phrases, there are many lexical gaps —*John gave a bone to the dog / *John boned the dog; John showed a robin to his aunt / *John robined his aunt* — and the constraint in both cases is that the head of the PP-phrase or the DO here must have both noun and verb entries in the lexicon.

The second criterion, (2) *reversability of phrase ordering*, so as to bring either the IO or the DO closer to the V, is the hallmark of bitransitives in English and many other variable-order languages: unlike the case with PP-phrases, however, application of this criterion serves to clearly segregate two types of bitransitives in terms of the *direction* of transfer, away *from* the Actor or *toward* the Actor (i.e., a kind of deixis); thus, for *give* and *sell*, dative movement is acceptable (e.g., *John gave Mary the book, John sold Mary the bike*), whereas for *take* and *buy*, similar movement shifts the meaning (*John took the book from Mary* / *John took Mary the book*; *John bought the bike from Mary* / *John bought Mary the bike*). The fact that for directions *toward* the Actor what we call the basic (natural) structure is acceptable (*John rented the studio from Mary*), whereas the other structure is not meaning-preserving (*John rented Mary the studio*) is in itself evidence in favor of our hypothesis; it would be interesting to see if this difference is a language universal.

The third criterion, (3) *passivization* — which served most clearly to distinguish two types of PP-phrases (VP-absorbing vs. VP-nonabsorbing) — in the case of passives serves simply to reaffirm the typology established by *reversability* (criterion 2): thus, for transfers *away from* the Actor there are two acceptable passives — *the book was given to Mary by John* as well as *Mary was given the book by John* and *the bike was sold to Mary by John* as well as *Mary was sold the bike by John*; on the other hand, for transfers *toward* the Actor there is only one acceptable passive — *the book was taken from Mary by John* but not **Mary was taken the book from by John* and *the bike was bought from Mary by John* but not **Mary was bought the bike from by John*.[12]

There is, however, a critical difference between PP-phrases and bitransitives, and it shows up in application of criterion 2 (*reversability of phrase order*). In the case of PP-phrases, the preposition *must be retained* in the acceptable (even if less natural) phrase order — thus *Khrushchev banged on the table his shoe* but not **Khrushchev banged the table his shoe* and similarly *Mack put on the table the knife* but not **Mack put the table the knife*. In the case of bitransitives, on the other hand, *the preposition most naturally is deleted* in the transformation — thus *the boy gave his sister the kitten* but not naturally ?*the boy gave to his sister the kitten* and similarly *the boy gave his dog a bone* but not naturally ?*the boy gave to his dog a bone*. This difference shows up strikingly when we compare PP-phrases and bitransitives having identical surface structures; using our earlier example, locative *Tom brought to his desk the book* but not **Tom brought his desk the book* as compared with bitransitive *Tom brought his roommate the book* but not **Tom brought to his roommate the book*. This contrast would seem to raise serious questions about the prepositional status of the *to* in bitransitives.

Cross-language linguistic evidence

In this section we make a number of predictions from the theory of cognizing and sentencing described above and test them against two sets of cross-language data on the surface structures of bitransitive sentences. The two sets of data are provided by Blansitt (1973) and by Sedlak (1975); both are based on materials available in the Stanford Language Universals Project and were published in the series titled *Working Papers on Language Universals*. The earlier paper by Blansitt has the advantage of covering a much larger number of languages (106) but the disadvantage of not breaking down the sample in terms of marking characteristics. The later paper by Sedlak provides the more detailed analysis in terms of marking characteristics but has the disadvantage of being based on a much smaller sample (44 languages). Part of the reason for this is that the latter used only languages for which field data included nouns in both direct and indirect object positions (as Sedlak notes, languages may have quite different rules for pronouns, as is the case for English). Before testing predictions, however, some methodological preliminaries are in order.

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[12] Note that, in the *toward* Actor cases, deletion of the preposition in the ungrammatical passive leaves an acceptable sentence, but again the meaning is shifted to an *away from* Actor reading.

Both authors give primary consideration to the prototypical bitransitives par excellence — in which both Actor and Recipient are humans and the transferred object is inanimate — and both distinguish between fixed and variable order (of DO and IO with respect to the verb) languages, although Sedlak admits that this distinction is somewhat arbitrary (since even languages with one order clearly dominant will usually have some other order or orders available). In correspondence with both Sedlak and Greenberg, the present writers raised the question of just what *are* the criteria here. Ideally, one should have some sort of frequency count of alternative orders from running texts, but such are obviously not available in existing grammars, particularly those produced by field workers dealing with languages spoken by small populations. Blansitt gives a priority-ordered set of four criteria, essentially as follows: if two or more slots in the surface structure can be used for the IO, then (I) that used for the nominally manifested IO (as opposed to the pronominally expressed), (II) that used for the prototype bitransitive IO (as opposed to nonprototype), (III) that used with unmarked IO (as opposed to marked), and (IV) that slot used for personal and/or animate IO (as opposed to nonpersonal and/or inanimate) will be considered the *primary* indirect object slot. Apparently Sedlak used essentially the same criteria.

In dealing with what are necessarily statistical universals — 'necessarily' because there are competing dynamics at work — there is an advantage in large numbers, here of languages. We had therefore hoped that it would be possible to combine the Blansitt and Sedlak samples (only counting the 26 languages which appear in both samples once, of course). However, this proved to be impossible. On close examination it became clear that there were differences in the classifications of quite a number of languages as being either fixed or variable in DO/IO order, and, if the former, which order was in fact dominant.[13] So there must have been some differences between the two analysts, at least in emphasis on the several criteria. This is unfortunate, because, with the Sedlak sample containing 18 languages that were not covered by Blansitt, the total of potentially usable languages would have been at least 124. We had also hoped that it would be possible to test, across the full sample of languages, the hypothesis (see Prediction #2c below) that, as the semantic characterization of the DO shifts from an obvious part of the action (*give a kiss to*), through being a concrete inanimate object (*give a book to*), to being an adult human entity (*give one's sister to*), the probability of both the basic ordering (with DO closest to V) and the marking of the IO (Recipient) would increase. But, as Blansitt (1973:11) points out, ". . . most of the source grammars used . . . fail to mention or illustrate bitransitive clauses in which the direct object has a personal referent."

Predictions #1: ordering

The most obvious prediction from our hypothesis about the cognizing of bitransitives, of course, is (#1, a) that *more languages will have a dominant surface order in which O is closer than D to V than will have a dominant surface order in which D is closer than O to V*.[14] Table I(A) gives — for all languages in the Blansitt and Sedlak samples and regardless of the basic typologies of languages, fixed or variable ordering, or types of marking — the ordering of D and O in terms of their closeness to the verb. It should be noted that in Table I(A) and I(C) the frequencies of languages with variable D/O ordering are combined with those fixed-order languages for which D and O are equally close to V (that is, V falls between D and O, e.g. SOVD or SDVO). The argument here is that, since languages with O before S are very rare, the typical variations in order will involve rotations of D and O in relation to V (thus SVOD/SVDO, SDOV/SODV, VSOD/VSDO), and if variability in ordering is reasonably balanced — which should be the case if a language is classified as variable order — then O and D are about equally likely to be closest to V.

Inspection of Table I(A) shows that this prediction is clearly *not* borne out. Although there are variations between the Blansitt and Sedlak samples, if one averages the percentages for the two he finds that there are almost exactly 33.3% having D closest to V in the dominant order, 33.3% having O closest to V, and 33.3% variable or with D and O equally close to V. Is there any reasonable explanation for this null result? We can suggest two interrelated factors.

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[13] There are even a few cases where Blansitt and Sedlak disagree on the basic typology for a language, e.g., as SVO or SOV.

[14] Here, following both Blansitt and Sedlak, we shift to using the symbol *D* (dative) for *IO* (indirect object) and the symbol *O* (object) for *DO* (direct object).

TABLE I

DOMINANT ORDERING OF DIRECT (O) AND INDIRECT (D) OBJECTS IN TERMS OF CLOSENESS TO THE VERB (V) IN BITRANSITIVES

(A) *For All Languages in Samples Regardless of Basic Language Typology, Fixed or Variable Ordering, or Type of Marking*

SEDLAK DATA						BLANSITT DATA					
D		=*		O		D		=*		O	
N	%	N	%	N	%	N	%	N	%	N	%
15	(.33)	21	(.47)	9	(.20)	38	(.36)	21	(.20)	47	(.44)

(B) *For the Three Types of Languages with Fixed Dominant Ordering*

SEDLAK DATA							BLANSITT DATA					
D		=		O			D		=		O	
N	%	N	%	N	%		N	%	N	%	N	%
10	(.77)	1	(.08)	2	(.16)	SVO	25	(.50)	2	(.05)	23	(.45)
5	(.45)	1	(.10)	5	(.45)	SOV	8	(.28)	5	(.18)	15	(.54)
0	(.00)	0	(.00)	2	(1.00)	VSO	5	(.36)	0	(.00)	9	(.64)

(C) *For the Three Types of Languages, All Languages (Fixed and Variable)*

SEDLAK DATA							BLANSITT DATA					
D		=*		O			D		=*		O	
N	%	N	%	N	%		N	%	N	%	N	%
10	(.37)	15	(.56)	2	(.07)	SVO	25	(.41)	13	(.21)	23	(.38)
5	(.36)	4	(.28)	5	(.36)	SOV	8	(.27)	7	(.23)	15	(.50)
0	(.00)	2	(.50)	2	(.50)	VSO	5	(.33)	1	(.07)	9	(.50)

*These values include all variable-order languages.

TABLE I (CONT.)

SEDLAK AND BLANSITT DATA (%'S) COMBINED

	D	=*	O
SVO	39.0%	38.5%	22.5%
SOV	31.5%	25.5%	43.0%
VSO	16.5%	28.5%	55.0%

(1) It has already been noted that, for the prototypical bitransitive, the semantic codings of $^{-}$Animate for O and $^{+}$Human for D are sufficient for distinguishing them without requiring either ordering or marking rules. It is the *non*prototypic bitransitives — particularly with $^{+}$Human DO or $^{-}$Human (but still animate) IO — that put the pressure on languages to develop either ordering or marking rules. And once such rules develop in a language, they are likely to spread stylistically through the system. But, having originated in special cases, one might expect such ordering or marking rules (or both) to be quite variable across languages.

(2) If one accepts as valid for the sake of argument our hypothesis that in bitransitives the so-called DO is really a part of the relation (VP) and that the 'real' DO is the so-called IO (the Recipient of the transfer), then we note that there is an inevitable competition between two dynamics of sentencing — on the one hand, to express the DO as close to the V as possible, since they are part of the same component of a simple cognition; but, on the other, to complete the essence of the simple cognition (S, V and the 'real' O in the case of SVO-type languages), leaving the remainder of the VP to be tagged on. Now if we imagine the dative to be the 'real' DO and look at where the dative (D) typically appears for each basic language type when our prediction about closeness of O to the verb fails, we note that for SVO types it is SVDO, for SOV types it is SDOV, and for VSO types it is VSDO. If we substitute O for the D as being the 'real' direct object and delete the O as being the still unexpressed remainder of the V, then we get the basic structure for each type back again — SVO, SOV, and VSO — as if the forward dative movement were, indeed, completing the basic structure of simple cogs in each case.

TableI(B) and I(C) break the Blansitt and Sedlak data down in terms of language types (SVO, SOV, VSO) with I(B) restricted to fixed-order languages and I(C) including all languages (again, in the latter case, with variable order languages combined with equally-close fixed order languages). But before exploring these tables we will make a prediction about language types and closeness of O to V, namely (#1, b) that *the tendency for O to be closest to V will be least for SVO and greatest for VSO, with SOV languages falling somewhere in between*. In presenting an outline of the general theory of cognizing and sentencing earlier (pp. 540-542) we indicated the need for a BUFFER and stated that when the sets of semantic inputs to or outputs from the LEXICON do not correspond to the 'natural' ordering in terms of which the OPERATOR is structured, *temporally mis-ordered sets of semantic information are transferred from the* OPERATOR *to the* BUFFER, *stored there briefly, and then transferred back to the* OPERATOR *for cognizing.* Needless to say, such transferring and storing must (in theory) increase processing difficulty and time.

If the relation in bitransitives (-- (DO) -->) is broken up by the dative movement transformation, then the inserted information must be 'buffered' until the VP is completed. In the following sentence pairs (presumed basic and transform for each language type), the barred lines indicate the assumed storage-in-buffer information.

	basic:	THE JANITOR GAVE A BATH TO HIS NEPHEW
SVO		├──────────┤
	transform:	THE JANITOR GAVE HIS NEPHEW A BATH
	basic:	THE JANITOR HIS NEPHEW TO A BATH GAVE
SOV		├─────────────┤
	transform:	THE JANITOR A BATH HIS NEPHEW TO GAVE
	basic:	GAVE A BATH THE JANITOR TO HIS NEPHEW
VSO		├────────────────────────┤
	transform:	GAVE THE JANITOR TO HIS NEPHEW A BATH

Here, it should be noted, we are not concerned with the presumed 'naturalness' psycholinguistically of SVO as compared with SOV and VSO, but rather simply with the holding-time between initiation and completion of the relation component (VP).

According to this analysis, such holding-time is shortest for SVO, somewhat longer for SOV, and much longer for VSO types. The small difference between SVO and SOV is the inclusion of the dative marker in the latter but not (as in English) in the former. Is there any justification for this? Inspecting Sedlak's illustrative sentences for his languages (which in many cases provide insufficient evidence, unfortunately), the first author found six SVDO languages in which the extraverbal dative marker seems to be dropped in the transform, like ENGLISH (COPTIC, DANISH, JAMAICAN CREOLE, SWAHILI, SWEDISH, and YIDDISH), and four in which it seemed to be retained (BAHNAR, FRENCH, GERMAN, and ITALIAN); for the three SODV variable languages for which illustrations are given, two seem to retain the dative marker (TELEFOL and TURKISH) and the other (KAPAU) proved indecipherable to C.E.O. For the fixed-order languages (see Sedlak 1975, Table 1), the evidence is stronger, although the numbers are still small:

for the ten SVDO languages, there is only one (MAMVU) in which the dative is extraverbally marked: for the five SODV languages, four retain extraverbal marking of the dative and the other (KEWA) is entirely unmarked. So combining variable and fixed-order languages, we have 16 of 20 SVO languages dropping the extraverbal dative marker in the transform as compared with only 2 of 8 for SOV languages.[15]

In any case, the data presented in Table I(B), for fixed-order languages, support the prediction (#1, b) made above: for both the Sedlak and Blansitt samples, SVO languages show the least tendency for O to be closest to V (16% and 45%, respectively), SOV languages a greater tendency (45% and 54%), and VSO the greatest tendency for O to be closest to V (100% and 64%, respectively) — although the number of VSO languages is very small, as would be expected. Table I(C) gives data for all languages, both fixed and variable in order (where variable languages are treated as having D and O equally close to V). The lower half of this table — averaging the percentages for Blansitt and Sedlak samples — clearly indicates trends which are consistent throughout: as we move from SVO through SOV to VSO types, the probability of languages having D closest to V goes steadily down and the probability of having O closest to V goes steadily up. This clearly fits the prediction made on the basis of assumed differences in time that information must be stored in the BUFFER when dative movement occurs in the three language types — but of course it may also fit other hypotheses.

Predictions #2: marking

There are three related predictions that can be made with regard to differential linguistic marking of D and O. The first is (#2, a) *that across all languages* (of all basic types and with both fixed and variable orders) *D will more frequently be marked than O*. This follows from the fact that it is D which is prototypically $^+$Human and hence potentially more confusable with the $^+$Human subjects of bitransitive sentences. The fact that D is also the 'real' direct object (M2) in bitransitives, according to our theory, should provide competition with this prediction. The second prediction is (#2, b) *that in transforms of the basic or 'natural' cognitive structures in which D is moved closer to V there should be greater relative marking of D than when O is closer to V* (*the 'natural' form*). The reasoning here is that — English to the contrary — surface sentence structures paralleling underlying cognitive structures, being more 'natural' are less likely to be cued with marking than transforms from the underlying cognitive structures. Here again — for SVO and VSO languages, but not for SOV — there is the potentially competing fact that the dative movement will also bring D closer to the typically human subjects of bitransitives. The third prediction is (#2, c) *that both fixed ordering and marking of D rather than O should be more likely for the non-prototype* (with ⁺Animate-Human O) *than for prototype bitransitives*. This again follows from the potential confusability of one object (the DO) with the other (the IO) of a bitransitive.

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[15] Sedlak defines *extraverbal* marking (where the -verbal refers to verbs, not to *words*) as that which is outside of the verb boundary (pre- and post-positions, case endings in NPs, calcified serial verbs); *intraverbal* marking is within the boundary of the verb as a word form (hence, affixing, concord, cross-reference). Forms like the *to* in English are thus extraverbal.

For testing these predictions we will rely mainly on Sedlak (1975), even though his sample of languages is much smaller, because he classified languages of all types according to either extraverbal or intraverbal marking (or no marking) of D and/or O.[16] Table II presents the frequencies of such markings (by number of languages, not giving percentages, in this case) as a function of closeness of D vs. O to V. To test the first marking prediction — that D will be marked more frequently overall than O — we need merely add the sums of columns in the table. For fixed-order languages (of all types combined) the ratio of D to O marking is 12/8 for extraverbal and 7/3 for intraverbal; the sum for both types of marking is

TABLE II

EXTRAVERBAL AND INTRAVERBAL MARKING OF D VS. O
AS FUNCTIONS OF CLOSENESS TO V
FOR FIXED AND VARIABLE ORDER LANGUAGES
(FROM SEDLAK, 1975, TABLES 1 AND 2)

FIXED ORDER LANGUAGES

	Extraverbal		*Intraverbal*		*Sum*	
	D	O	D	O	D	O
D closest to V:	5	2	5	2	10	4
Equally close:	1	1	1	0	2	1
O closest to V:	6	5	1	1	7	6
Sum:	12	8	7	3	19	11

VARIABLE ORDER LANGUAGES

	Extraverbal		*Intraverbal*		*Sum*	
	D	O	D	O	D	O
Equally close:	13	5	1	1	14	6
Grand Sum:	25	13	8	4	33	17

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[16] See footnote #15 above for definitions of extraverbal and intraverbal marking.

thus 19/11, D greater than O for fixed-order languages. For variable-order languages (which are combined as cases where D and O are equally close to V), the ratios are 13/5 for extraverbal and 1/1 for intraverbal, the sum being 14/6 favoring marking of D over O. The sums over both fixed and variable languages are 25/13 for extraverbal and 8/4 for intraverbal, yielding a grand-sum ratio of 33 languages with D marked and 17 with O marked.[17] Quite consistemtly throughout, D is marked about twice as often as O, thus clearly confirming this prediction.

In testing the second prediction (#2, b) — that the relative marking of D vs. O should be greater for transforms that place D closer to V than for the 'natural' forms with O closer to V — we can only use the Sedlak data for fixed-order languages in Table II. Summing extra- and intraverbal marking for the three rows of the upper table we find a ratio of 10/4 favoring marking of D when D is closest to V, a ratio of 2/1 when D and O are equally close to V (but with only two languages contributing), and a ratio of 7/6 (about equal) when O is closest to V. This trend fits the hypothesis, but the numbers involved are very small.

Although no tabularized data are offered, Blansitt (1973:20) states as one of his potential universals the following: "III. If a language of the unitransitive SVO type has different relative orders depending on whether the indirect object is marked or unmarked, the order is SVDO with unmarked indirect object and SVOD with a marked indirect object" — in other words, like English. For some reason, Sedlak doesn't present any data where variable-order languages are categorized simultaneously in terms of type of marking and proximity of D vs. O to V (he does this only for fixed-order languages). Nevertheless, his Tables 1 and 2 have some marking evidence that is relevant to the present prediction. We note in his Table 1 that the 10 SVDO languages have almost exclusively *intraverbal* marking (and usually of D rather than O), whereas the five SODV languages have only *extraverbal* marking. Since D moves toward S in the former case and away from S in the latter case, the implication is that marking D intraverbally (that is, via affixing to the verb) serves to disambiguate the ⁺Human D from the ⁺Human S most adequately. Noteworthy in relation to this phenomenon for fixed-order languages is the fact that in Sedlak's data for 19 variable-order languages of all types (but mostly SVO) there is only one case of intraverbal marking (TELEFOL); all of the rest are either unmarked for both D and O (five languages) or are *extraverbally* marked (13 languages). It would appear that *variable* order, as compared with fixed order languages (above), rely heavily on signaling by pre- or postpositions and case markings on noun phrases.

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[17] In the cases where *both* D and O are marked in a language, these counted toward the frequencies of both D and O. It is worth noting that there are no cases in Sedlak's Tables 1

With regard to prediction (#1, c) — that either or both a shift to fixed ordering and marking of D rather than O is more likely for nonprototype bitransitives — we only have some observations by Blansitt (1973). He notes that in DIOLA FOGNY all possible orders of V, D, and O are permitted — *except* when O has an animate, human referent; then the *order* is restricted to two forms, DOV or VOD, both with O closest to V (1973:8). In two other languages, KIKUYU and BIROM, cited by Blansitt (1973:11), the order of V, D, and O is standard for prototypical bitransitives, but a different order is required when O has a *personal referent*; in both cases, the standard order is SVDO, but when D and O have personal referents the order is SVOD — note, again, with O now contiguous with V. These bits of evidence, although skimpy, are consistent with the prediction made.

Predictions #3: Use of O-marker for D

Blansitt's (1973:20) potential universal IV is that "if the direct object is marked in the unitransitive clause, the indirect object will be marked in the bitransitive clause", and, although he does not say so explicitly, it is clear from the data that in this case the direct object may or may not be marked. An extreme prediction from theory is (#3) *that if the direct object is marked in unitransitive clauses but is not marked in bitransitive clauses, then the same marker will be used for the indirect object in bitransitive clauses*. This far-out prediction was admittedly *post hoc* — after we found that Blansitt reported such cases — but it follows obviously enough from the theory: we have argued that the 'real' direct object in bitransitives is the prototypically ⁺Human Recipient; if this is the case, then logically it would follow that, as long as the O in bitransitives is not marked (unless differently than in unitransitive clauses), the same 'real object' marker should be applied to D in bitransitives. Of course there are many competing factors here — particularly the *semantics* of the O in both uni- and bitransitive (typically ⁻Animate) and of the D in bitransitive (typically ⁺Animate as well as ⁺Human) clauses.

It turns out that in SPANISH and some other languages (Blansitt 1973:18) the direct object is regularly marked in the unitransitive clause when the referent is definite and personal — thus getting around the semantic competition just mentioned. In SPANISH, animate nonhumans may also be so marked in unitransitives. As to the other languages,[18] along with SPANISH, Blansitt lists KHASI, GUARANI, SOUTHERN SIERRA MIWOK,

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and 2 (fixed and variable languages) where a language has *both* extra- and intraverbal marking (e.g., marking D extraverbally but O intraverbally), as if these processes are mutually exclusive.

[18] These others are listed in a pre-publication draft on p. 16, but for some reason are omitted in the 1973 published paper.

TARASCAN, BANGRU, GUJARATI, REMO, TIGRINYA, LAMANI, NEPALI, and TAHITIAN, and according to Sedlak's analysis (1975:133), GUNBALANG should be added to the list, all of which use the *same* O marker for D in bitransitive sentences. This is 13 languages, a goodly number, and they are spread across all three basic language types. We consider this very strong evidence for our hypothesis — a direct surface manifestation that the so-called IO is functioning psycholinguistically as the DO in bitransitives. One recalls that old TV show in which each segment ended with the question: "Will the real Joe Blow please stand up?" These languages seem to be cases where the 'real' direct objects in bitransitives do stand up!

Predictions #4: Incorporation of nouns in verbs

Sedlak (1975:135-141) has an entire section devoted to evidence on the incorporation of nouns in verbs. Such incorporation would seem to be a hallmark of closeness of relation in cognizing (the most direct linguistic parallel of the experimental studies on tightness of cognitive binding of O vs. D with V, to be reported in the next major section). The prediction from theory here is obvious: (#4) *that in bitransitives incorporation of the direct object in the verb should be a frequent phenomenon* (in languages that display verb incorporation) *but incorporation of the indirect object should be very rare*. This follows from the fact that DO is assumed to be part of the relation in bitransitives, symbolized by – – (DO) – – >, whereas IO is a separate component (M_2) of simple cognitions; to incorporate IO in the VP, leaving an independent DO, would be to simultaneously disrupt what should be a single component in cognizing and fuse what should be independent components.

Sedlak notes that both Alfred Kroeber and Edward Sapir were early observers of this phenomenon in American Indian (as well as other) languages. Sapir (1911:257) defines noun incorporation as the "compounding (of) a noun stem with a verb. . . ." According to Sedlak, Sapir reports that incorporation of direct objects is the most common phenomenon in sentences with transitive verbs (where instrumentals, locatives, etc., may also be compounded), but he makes no mention at all of indirect object incorporation. On the other hand, indirect object *pronouns* can be incorporated in the verb, but relatively rarely according to Sapir (see below, under Prediction #5).

Analogous to incorporation is the process of verb-noun compounding in MANDARIN, which Chao (1968:359) defines as "a combination of two or more words bound together to form one word" — examples, *gaw-lao* (announce old age, retire), *chiuh-shyh* (depart from the world, die), *daa-tih* (strike sneeze, to sneeze). Chao lists a number of conditions which must be satisfied if a sequence is to be considered a compound (see Sedlak

1975:139-140 for details), the last of which is lexicality (specialization) of meaning — which, Chao says (1968:423) is "not only non-structural in nature, but also more difficult to apply, since it is a factor which admits of difference of degree." We might note that the word-like-ness of nominal compounds in English (like *real estate* and *stumbling block*) has been amply demonstrated in tachistoscopic studies by Osgood and Hoosain (1974), and such compounds also conform to Greenberg's (1957) definition of the word as a linguistic unit — that it resists insertion within its boundaries (thus *he's an old stumbling block* but not *he's a stumbling old block*, without changing meaning). Sedlak points out that among the very numerous examples of verb-noun compounding that Chao cites, there is not a single case involving indirect objects, and he concludes that "although MANDARIN is noted for the richness of formation of various types of compounds . . . there is no type in which the D noun may be incorporated" (1975:140). The available evidence thus strongly supports Prediction #4.

Prediction #5: Basic less constrained than transform

Sedlak (1975:130-131) presents convincing cross-linguistic evidence that D is the marked category and not O. His reasoning was that if D is marked either obligatorily or optionally and O is unmarked, or if D is marked obligatorily and O only optionally, then for that language D is the marked category. The converse are the conditions for considering O as the marked category. Applying these rules, he finds that for fixed-order languages 39% are D-marking and only 8% O-marking and for variable languages 53% are D-marking and only 7% O-marking (the remainders are languages which mark both or neither). Looking at only SVO and SOV languages, which have reasonable numbers, we find that of the eight D-marking languages six have D closest to V while only one (CHIPEWYAN) has O closest to V (one having both equally close to V). Marking generally seems to serve as a sort of 'flag' that there has been deviation from the cognitively natural order — witness the *be verb by* of the passive, for example. The prediction being made here is more general than marking, however, namely (#5) *that the surface form of bitransitive which corresponds to the assumed underlying cognitive structure will display fewer lexical and syntactic constraints than the transformed surface form.* This means that the SVOD, SDOV, and VSOD (with O closest to V) for the three language types should be less constrained than their SVDO, SODV, and VSDO transforms.

We have already seen some evidence of this for English above: the basic SVOD form can be used for both transfers *away from* and *toward* the subject (*John gave the book to Mary* and *John took the book from Mary*) but the transformed SVDO can only be used for the prototypical transfer *away from* without changing meaning (*John gave Mary the book* but not *John took Mary the book*); similarly, passivization of the SVDO form for transfers *toward* the subject are ruled out (**Mary was taken the book from by John*). To these constraints on the transform bitransitive we may now add the following: (1) *Lexical constraints*. The verbs that the transform will accept are typically high frequency, irregular verbs, often of Germanic origin (*give, lend, send, bring, take*); many other transfer verbs, often relatively infrequent and regular, are acceptable in the basic but not in the transformed structure — examples, *John donated the book to the club / *John donated the club the book*; *John described the book to Mary / *John described Mary the book*; *John surrendered the loot to the police / *John surrendered the police the loot*; and so forth. (2) *Compound verb syntactic constraints*. English contains many verb + particle compounds which clearly function as single verbs (*call up, plead with, give away*, etc.) and many, by all semantic criteria, are verbs of transfer having three arguments, like *the millionaire gave away his fortune to needy people*. Yet such compound verbs of transfer can be used in the basic bitransitive form but seldom, if ever, in the transform — witness **the millionaire gave away needy people his fortune*. Some other examples: *Sam gave up his life for his mistress / *Sam gave up his mistress his life*; *Sarah made up a lunch for her husband / *Sarah made up her husband a lunch*; *John turned over his wallet to the thief / *John turned over the thief his wallet* / or **John turned the thief over his wallet* (!).

What about cross-linguistic evidence for greater constraints on D-closer-to-V transforms? Apparently either this differential-constraint hypothesis was not particularly prominent among the concerns of Blansitt or Sedlak or the available illustrative sentence materials were not sufficiently detailed. The only directly relevant observations are made by Blansitt (1973:10-11): in some languages (he specifies a subgroup of SVO-types which includes CHI NYANJA), while prototype bitransitive clauses have D unmarked, nonprototype bitransitives have the D marked prepositionally; in LUO (also SVO) the D is unmarked with prototypical *give*, but other bitransitive verbs occur with D marked by the preposition *ni*. He notes that these phenomena are like what we termed "lexical constraints" above for ENGLISH. He includes as nonprototypical cases where the O has personal reference (e.g., KIKUYU, ACOOLI, and BIROM) and the D must be marked — which we considered under Prediction #2, c.

There is another type of constraint which appears (3) *when pronouns are substituted for nouns in bitransitives*. In correspondence during the course of our research, Greenberg (letter to C.E.O. dated 6/21/74) made the following insightful comment: "Most of the time pronominal and nominal constructions have the same order. I believe that the frequency of pronominal elements as either indirect or direct objects in texts is probably quite high. For example, looking in the OED to find early instances of word order, most of the examples involved pronouns, particularly for the indirect object. What I am driving at is that perhaps languages model noun constructions after pronoun constructions in this area. It is therefore important to look at languages in which the orders differ. I think these will generally show that pronouns tend to stay near the verb as compared to nouns." In English this appears to hold more clearly for O than for D, as shown in the following,

John gave the book to Mary	John gave Mary the book
John gave the book to her	John gave her the book
John gave it to Mary	*John gave Mary it
John gave it to her	*John gave her it

where the basic or 'natural' order (SVOD) is developed on the left and the transformed or 'unnatural' order developed on the right. We note that all basic forms, with O closest to V, are clearly acceptable; on the other hand, for the transformed order, with D closest to V, only those where O is nominal are acceptable — both cases where pronominal O is separated from V are strange at best.

What about other languages? Aside from the comment that pronominal orders may differ from nominal orders (and dependence on the latter as prior evidence to the former in classification), Blansitt (1973) mentions pronominal examples only incidentally, not contrastively. In his introduction (1975:124), Sedlak says "pronouns have been excluded, not for any theoretical reason, but because of weakness of coverage in the sources." However, Sedlak (1975:136) does offer us a significant quotation from Sapir (1911:250): "A very large number of American, as of non-American, languages make use in the verb of affixed elements of pronominal signification; they are, as regards their syntactical use, very commonly subjective, less frequently, though by no means rarely, also objective, and still less commonly they indicate also dative, ablative, or other case relations." For present purposes, we note that verb affixing of pronominal O is more frequent than affixing of pronominal D — quite in keeping with our expectations. Greenberg (6/21/74 letter) gives some interesting data on Ancient Egyptian, which was a prepositional VSO language with nominal or pronominal D always marked with a preposition: when both O and D are nouns, the order is VSOD (*gives king gold to-servant*); if one or the other is a pronoun, it moves ahead of the subject (either *gives it king to-servant* or *gives to-me king gold*); if both are pronouns, then both move before the subject, with D being before O (*gives to-me it king*). This pattern certainly fits Greenberg's notion that pronouns gravitate toward the verb, but the last example (both D and O pronouns) goes contrary to our expectations.[19] A full cross-language exploration of this pronominal/nominal contrast in terms of both ordering and constraints should be a rewarding enterprise.

Working first with questionnaire and then with interview data from speakers of languages other than English, the second author has made such a contrastive analysis of 13 SVO and 12 SOV languages. She probed possible word orders — first with D and O both nominal, then with D pronominal, then with O pronominal, and finally with D and O pronominal. Table III gives a summary of her findings.[20] For SVO languages: when both objects are nouns, O tends to be next to V more often (62%) than D (45%); when only D is a pronoun, there is a 50/50 chance of either O or D being next to V; but when only O is a pronoun, or when D and O are pronouns, O is definitely preferred next to V (87% to 47% and 76% and 36%, respectively). For SOV languages the situation is quite different: although there is a consistent preference for O rather than D next to V, it is rather small in magnitude and doesn't mirror the patterning for SVO languages. Actually, we would have expected the difference favoring O next to V to be *greater* under all conditions for SOV than for SVO languages. Let us again assume that, in bitransitives, D is the 'real' direct object; whereas for SVO languages the tendency to keep O next to V must compete with the tendency to complete the components of the simple cognition (thus $SV\underline{D}O$, where $\underline{D}$ is 'really' O), for SOV languages both tendencies work the same way (thus $S\underline{D}\bar{O}\bar{V}$, where $\underline{D}$ is the 'real' O and $\bar{O}\bar{V}$ together equal V, yielding the basic SOV). So either there must be yet other tendencies working or this result should count against the theory proposed here.

Experimental psycholinguistic evidence

In this section we report three experimental studies, each of which — in a somewhat different way — is designed to test the basic hypothesis that the DO is more tightly bound to the verb than the IO. This, of course, follows directly from the structure of the simple cognition postulated for bitransitive sentences, based on pre-linguistic experience with three entities participating in a single (transfer) action event — symbolically

$$[M_1 -- (DO) --> M_2].$$

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[19] However, it will be recalled that Greenberg himself pointed out that VSO is the only language type in which the predicate is split by the subject, and he suggested that there should be a competing tendency to shift from VSO to VOS; by the same argument, if D is the 'real' *O*, then the VDOS order would represent fulfillment of this tendency.

[20] It should be noted that speakers of the same language were not always consistent in their orderings — as is the case with English speakers, of course.

TABLE III

CLOSENESS TO THE VERB OF NOMINAL AND PRONOMINAL DIRECT AND INDIRECT OBJECTS

	SVO Languages (N = 13)		*SOV Languages (N = 12)*	
	O NEXT TO V	D NEXT TO V	O NEXT TO V	D NEXT TO V
O AND D NOUNS	18/29* (62%)	13/29 (45%)	17/31 (55%)	14/31 (45%)
D PRONOUN	15/24 (63%)	15/24 (63%)	17/32 (53%)	14/32 (44%)
O PRONOUN	13/15 (87%)	7/15 (47%)	13/25 (52%)	12/25 (48%)
O AND D PRONOUNS	11/14 (76%)	5/14 (36%)	12/21 (57%)	9/21 (43%)

* The denominators represent the total number of tokens of different orders in the sample of languages; the fact that the number is larger than the number of languages sampled simply reflects the fact that many languages contributed more than one order (number of speakers for languages varying).

Experiment I gives the subjects bitransitive (and other) sentences and, after interpolated activity, presents the sentence subjects as cues for recall;[21] the hypothesis is that, given recall of the verb, there will be a greater conditional probability of recalling the DO than the IO, regardless of the form (basic SVOD or transform SVDO) presented. Experiment II modifies the procedure in several ways, but mainly by giving the sentence subject *with* the verb as the cue for recall; the hypothesis is no longer a conditional probability, but the simple probability that recall of DO will be higher than recall of IO. In both of these experiments two additional predictions are made: (a) that correct recall of the lexical content and syntactic order will be greater when the sentences are presented in basic SVOD form than in transformed SVDO form; (b) that shifting the recalled order to the alternative form will be greater when given the transform (SVDO) than when given the basic (SVOD). Experiment III uses a probe technique in which, after a set of sentences has been presented, all of the key content words are presented singly and in random orders, with the subjects instructed to give the first other word (of the same sentence) that occurs to them; the major hypothesis is that V will be more likely to elicit DO than IO and, reciprocally, DO will be more likely than IO to elicit V.

Experiment I[22]

Subjects. Twenty-four male undergraduates at the University of Illinois served as subjects. Their participation in psychological experiments for a certain number of hours was a condition for satisfactory completion of the introductory psychology course. The authors have some reservations about the 'subject pool' method; the experimenter has to create adequate motivation, or else he's likely to face rote memorizing at one extreme or almost complete 'forgetting' at the other. We tried to create interest in all of these experiments.

Materials. Sets of 36 bitransitive and 36 prepositional three-entity (NP) sentences were generated by the authors. No results for the latter will be reported here; the prepositional sentences (e.g., *the actress put the baby on the trunk*) can be considered simply as buffer materials — which they were, of course, reducing the monotony of SVDOs and SVODs. One of the problems we faced with the bitransitive sentences was avoiding the usual predictability on either semantic or associative grounds of D or O from the subject or verb of a sentence. As a subject, *the butcher* would be predictive of an O like *leg of lamb*; similarly, a verb like *lent* would be highly predictive of *money* as the O; and *the judge* as subject would be predictive of a D like *the jury*. On the other hand, a conspicuously high degree of *un*predictability would be likely to make the experimental sentences relatively meaningless, very hard to recall, and hence lower subject motivation.

On intuitive grounds (those of Osgood and Tanz, of course), the sentences had to be constructed within these upper and lower boundaries of interconstituent predictability. It is this constraint that makes our sentences the rather strange and often dull sequences they are — examples: *the florist gave a cat to the children*; *the customer showed the grocer the map*; *the teacher threw a towel to the cook*; *the queen made the gardener five shirts*; *the police took the tents to the musicians*; *the foreigners taught the priests the rules*. Such sentences neither make good sense, by confirming expectations, nor make particularly interesting sense, by significantly jarring expectations. As was suggested by the examples above, half the sentences were the SVOD versions and the other half were SVDO versions of the same content.

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[21] The term *subject* is an unfortunate homonym for psycholinguists!

[22] This study was conducted by Chris Tanz.

Procedures. Each subject was presented a total of 18 bitransitive and 18 prepositional sentences. Half of the bitransitives for each subject were in the SVOD form and half in the SVDO form — but randomized, of course, and interspersed with prepositional sentences. Across subjects, each particular sentence content was presented in one form to half the subjects and in the other form to the other half. The 36 individual sentences (bitransitives and prepositionals interspersed) were the materials for two alternating tasks: Task 1 was *probe verification* (after the sentence had been presented, and the subject had repeated six random digits, one word from it (V, D, or O for bitransitives) was flashed on a screen and the subject merely had to say "yes" or "no" to indicate whether that word had in fact been in the just-presented sentence); Task 2 was *cued recall* (the subject of the sentence [e.g., *the florist*. . .] that had been presented *just prior* to the probed sentence was flashed on the screen, and the subject had to complete the original sentence as best he could). The probe task, including the repetition of six random digits, can be considered as interpolated activity for present purposes — to increase the difficulty of recalling the prior sentence.[23] The recursive interpolation of these two tasks is shown in the following partial sequence, where A and B indicate cycles:

A.1. present sentence 1;
A.2. present sentence 2;
A.3. filler activity (repeating 6 random digits);
A.4. present probe for sentence 2;
A.5. present cue for recall of sentence 1:

B.1. present sentence 3;
B.2. filler activity;
B.3. present probe for sentence 3;
B.4. present cue for recall of sentence 2; ETC.

The sentences were presented aurally via tape in order to preserve the sequential unfolding of sentences in time. The probes in the verification task and the subject NP cues in the recall task were presented on slides. The experimenter wrote down the subjects' sentence completions during the intervals between cycles.

Results. The major hypothesis for this experiment was that the contingency in recall between verb and direct object should be greater than that between verb and indirect object, thus *that given recall of V, the conditional probability of also recalling O should be greater than that of also recalling D*. However, such a tendency might exist independent of tightness of cognitive binding to the verb; it could be attributable to confusability, semantic and/or imagery factors: (a) given the fact that, for all of the bitransitives used here, both S and D are $^{+}$Human, there are nearly twice as many confusable D-like nouns as there are O-like nouns ($^{-}$Human in all but two cases); (b) all D being varieties of humans (*nephew, aunt, children, professor, engineer, doctor, cook, gardener*, etc.), the D set is less differentiated semantically than the O set (*bath, keys, cat, dance, letter, rules, five shirts, robin*, etc.); (c) the NPs in the O set seem generally more imageable (*towel, clock, tents, turkey*, etc.) than the NPs in the D set (it is a bit difficult to *differentially* image an *engineer*, a *doctor*, a *dentist*, a *professor*, a *mayor*, and so on). Therefore, to rule out such contaminating factors, it is necessary to verify a second hypothesis — namely, *that if V is* NOT recalled, there should be no differences in the recall of O vs. D.

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[23] Actually, the original intent of Task 1 (probe verification) was to see whether O or D was more quickly and accurately identified; but, on mature theoretical consideration after the experiment was underway, it became apparent that we could predict both ways! And in any case — probably given the reversing left-to-right orders of D and O — and the operation of primacy and recency effect — there were no significant differences.

With 24 (subjects) × 18 (sentences), we have a total of 432 sentence recalls for analysis. Unfortunately for our purposes — and perhaps indicative of the motivations and strategies of 'subject-pool' subjects — 44% of the sentence recalls were perfect (rote memorizing?) and 29% were cases of total forgetting, leaving us with only 27% of the data, the cases of partial recall, to work with. Table IV gives a summary of the hypotheses-relevant information: the data for recall of O vs. D *when the verb is recalled* are in Table IV(A) — broken down in terms of (1) perfect recall of V, (2) "recall" with synonym substitution (e.g., recalling *gave* as *handed*), and (3) 'recall' only in the sense of retaining the bitransitive structure (e.g., recalling *threw* as *gave*); Table IV(B) gives the data for recall of O vs. D *when the verb is* (1) *incorrectly recalled* (e.g., recalling *got* as *opened*) or (2) *omitted entirely*. The numbers in the table do not sum to 118 (the total of partially recalled items) because all cases where D and O were both changed or forgotten *or* both correctly recalled, even though the verb wasn't, are omitted. In this table 'change' in D or O

TABLE IV

DIFFERENTIAL RECALL OF DIRECT (O) AND INDIRECT (D) OBJECTS
AS A FUNCTION OF RECALL OF THE VERB

A: *If the Verb Is Recalled*

(1) *Perfectly Correctly*:	CHANGE D	17	CHANGE O	10
	FORGET D	16	FORGET O	4
(2) *With Synonym Substitution*:	CHANGE D	3	CHANGE O	1
	FORGET D	3	FORGET O	2
(3) *Only with Retained Bitransitive*:	CHANGE D	4	CHANGE O	2
	FORGET D	4	FORGET O	1

B: *If the Verb Is Forgotten*

(1) *By Incorrect Recall*:	CHANGE D	3	CHANGE O	1
	FORGET D	3	FORGET O	4
(2) *Completely (Omission)*:	CHANGE D	0	CHANGE O	0
	FORGET D	3	FORGET O	2

is defined simply as non-omission (forgetting) when the semantics of the substitute fit the semantics for a D or an O, respectively (thus *the man* would be a 'change' for *the gardener* as a D and *the picture* would be a 'change' for *the map* for an O).

When the verb is *perfectly recalled* (A.l), we find 17 changes in D vs. 10 in O (which just misses significance at the .05 level of significance by the binomial expansion test — using a one-tail probability since we are predicting the direction of the difference) and 16 forgettings of D vs. only 4 forgettings of O (which is significant at the .01 level — i.e., would happen by chance only one time per hundred). The frequencies of changing and forgetting *when there is synonym substitution for the verb* (A.2), are too small for testing, but if we combine perfectly corrects with synonym substitutions we get 20 to 11 for D vs. O changes (barely missing the .05 level) and 19 to 6 for forgettings (which is significant at the .01 level). Combining all three grades of verb recall, we get 24 to 13 changes for D vs. 0 (significant at the .05 level) and 23 to 7 forgettings for D vs. 0 (significant at the .003 level). The first hypothesis that, given recall of the verb, the conditional probability of recalling 0 is greater than that of recalling D is supported. The number of instances *where the verb is incorrectly recalled or entirely forgotten* (11 and 5, respectively) is too small for any confidence. All we can say is that the sums of changing and forgetting of D and 0 (9 to 7) when the verb is not recalled are about equal and obviously not significant — this being at least consistent with our second hypothesis.

The prediction that form-shifts in recall would be greater from SVDO into SVOD than from SVOD into SVDO was also checked in the data for this experiment. Of the 387 sentences that were recalled correctly content-wise, only 45 involved shifts form-wise, and of these slightly more than half (24) were shifts from SVDO to SVOD as predicted and slightly less than half were from SVOD to SVDO (21). Sign tests for the 16 usable sentences (two were transfers toward the subject and hence nonreversible) across the 24 subjects yielded 6 favoring the hypothesis, 6 neutral, and 4 against; for the 24 subjects across the 16 sentences, 9 favored, 5 were neutral, and 10 were against the hypothesis. Neither of these distributions is significant, of course. The reason for this prediction failure is probably the small numbers; that — with only one sentence and its probing intervening between presentation and recall — the subjects adopted a verbatim memorizing strategy, and when this failed they either partially recalled (116 sentences) or shifted (only 45 sentences).

Experiment II[24]

One of the problems with Experiment I was that — with only one other sentence, repeating 6 random digits, and responding "yes" or "no" to the probe interpolated — the proportion of perfect recalls was too high (nearly 50%). Another was that, given the great semantic similarity of verbs of transfer, the verbs were often forgotten or substituted for in the recall task, thereby reducing the number of partial recalls available for making the primary conditional probability tests. The present experiment was an attempt to remedy these problems, while testing essentially the same hypotheses: (1) that, with *both* sentence subject and verb given as cues, the probability of correctly recalling O will be greater than that for recalling D; (2) that the probability of (a) correct recall of sentences and (b) of keeping (not shifting) the form of the bitransitive in recall will both be greater when presented the SVOD basic form than when presented the SVDO transform.

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[24] This study was planned jointly by the authors and Quentin Schultze, who then conducted the experiment. We wish to thank him for his assistance on this research.

Subjects. Students taking introductory psychology and required to serve for a number of hours as subjects in experiments were again the native speakers used in this study. A total of 20 subjects was planned, but three failed to show up at the appointed time; the subject sample is presently being expanded.

Materials. The same sets of bitransitive and prepositional sentences as used in Experiment I were also used here, except that two of each type were eliminated in order to make the total divisible by eight. The subjects were divided into four groups: each group received four sets of eight sentences each, thus getting a total of 32 sentences — 16 bitransitives and 16 prepositionals intermixed within the 8 sentences in each set or block (see Procedure below). As before, our interest here is only in the bitransitive sentences: in each block a subject would be exposed to 4 bitransitive sentences, 2 in the SVOD form and 2 in the SVDO form; thus, over the entire four blocks, a subject would get 16 bitransitives, half in the SVOD and half in the SVDO form. Over the total experiment, each sentence is given to half the subjects in one form and to the other half in the other form.

Procedure. To the eight experimental sentences in each block were added one (nonbitransitive, nonprepositional) buffer sentence at the beginning and two buffer sentences at the end; this was done to reduce primacy and recency effects. The experimenter read each list of 11 sentences with natural intonation and speed, pausing briefly after each sentence. Then the subjects were given a list of the sentence subjects and verbs *in the same order that their sentences had been read*, each such cue being followed by two, clearly separated blank lines, e.g.,

The janitor gave ______________ ______________.

The purpose of this was to clearly indicate that two noun phrases were missing and also to eliminate the difficulties posed by modifiers of the heads of these phrases (e.g., *her clock, his nephew, some books, one turkey*) and the give-away preposition *to* if only the head nouns were replaced by blanks. In the example above, regardless of which bitransitive form he had heard, the subject could write

The janitor gave his nephew a bath.

or

The janitor gave a bath (to) (to) his nephew.

in the blanks, from which both correctness of recalls and shifts could be determined. Two of the four groups of subjects heard their sentence-blocks and were given their cues in one order (Group IA getting one D-O or O-D structure and Group IB the reverse for each sentence) and the other two groups heard their sentence-blocks and got their cues in the reverse order, the first-heard sentence now being what had been last-heard (Group IIA again getting one order of D-O and Group IIB getting the reverse form). This was done to further minimize primacy and recency effects across the entire subject sample.

Results. Table V presents a summary of the results for testing the several hypotheses indicated above. In this case, the results are reported in terms of the numbers of sentences (across the 17 subjects) or the numbers of subjects (across the 16 sentences) supporting (+), refuting (–) or balancing out (O) for each hypothesis.

TABLE V

TESTS OF PREDICTIONS IN EXPERIMENT II

Hypothesis 1: That O Will Be Recalled Better Than D Overall

FOR 16 SENTENCES ACROSS SUBJECTS			FOR 17 SUBJECTS ACROSS SENTENCES		
+*	0*	–*	+	0	–
5	7	4	5	8	4

Hypothesis 2(a): That With SVOD Presented There Will Be More Correct Sentence Recall (As Given) Than With SVDO Presented

FOR 16 SENTENCES ACROSS SUBJECTS			FOR 17 SUBJECTS ACROSS SENTENCES		
+	0	–	+	0	–
10	1	5	12	0	5

Hypothesis 2(b): That With SVOD Presented There Will Be Fewer Shifts in Order (from That Given) Than With SVDO Presented

FOR 16 SENTENCES ACROSS SUBJECTS			FOR 17 SUBJECTS ACROSS SENTENCES		
+	0	–	+	0	–
9	3	4	5	5	7

Hypothesis 2(a) and 2(b) Combined: That With SVOD Presented There Will Be Both More Correct Recalls and Fewer Shifts Than With SVDO Presented

FOR 16 SENTENCES ACROSS SUBJECTS			FOR 17 SUBJECTS ACROSS SENTENCES		
+	0	–	+	0	–
9	4	3	9	2	6

* In this summary, + indicates the *N* (of sentences or subjects) supporting the hypothesis, and – the *N* refuting the Hypothesis; O indicates the *N* where equal numbers (of subjects or sentences) supported or refuted the hypothesis.

Hypothesis 1 —*that O will be recalled better than D overall*— yields a near-perfect null result (pure chance), and the reason is not hard to find. Given both the subject and the verb, experimental subjects typically either got both object NPs (perfect recall) or forgot both; in fact, with 272 recalls (17 subjects × 16 sentences), there were only 21 cases where a subject got one object phrase correct and the other wrong — which is not much to work with.

So we turn to Hypothesis 2, which pivots on which form of bitransitive was presented to a subject (SVOD or SVDO) for subsequent recall. The theoretical rationale is that — since a bitransitive sentence must either correspond to the basic [M_1 – – (DO) – – > M_2] cognitive structure or must be shifted into this structure in order to be comprehended — SVOD presentations should be easily recalled (in that order) while SVDO presentations should tend to be recalled in the SVOD form. Hypothesis 2 (a) — *that with SVOD presented there will be more correct sentence recall* (in the form given, that is) *than with SVDO presented* — clearly comes out in the predicted direction (just barely missing the .05 level of significance because of the small *N*s) for both sentences across subjects and subjects across sentences. Hypothesis 2 (b) — *that with SVOD presented there will be fewer shifts in order of O and D objects* (from that given) *than with SVDO presented* — is supported for sentences across subjects (again, just missing the .05 significance level), but clearly is *not* for subjects across sentences. From inspection of the data, it is evident that individual subjects differ quite markedly in what appears to be stylistic preference for one form or the other; for the five subjects who support Hypothesis 2 (b), the proportions of sentences recalled in SVOD form are 14/16, *7/15, 11/16, 4/4, and 11/15 (where the denominator is the total number recalled); for the seven subjects who refute the hypothesis, the proportions of SVOD are 5/14, 3/7, 3/10, 4/13, 4/10, 6/15, and *4/6 — a bimodal type of distribution (with the two exceptions, starred) of performance when one considers that equal numbers of SVOD and SVDO were presented.

The last part of Table V combines Hypotheses 2 (a) and 2 (b) — *hence, that with SVOD presented there will be simultaneously more correct recalls and fewer shifts than with SVDO presented*. Supporting the theory (+) in this case means that, for either sentences across subjects or subjects across sentences, both of these predictions will be satisfied and refuting (−) means that both predictions will go the wrong way. The results for sentences again just barely miss the .05 level in support of the theory and the results for subjects are at least in the right direction.

The most tightly-controlled test of the theory is to compare the effects of SVOD vs. SVDO presentations *of the same sentences* (content-wise) upon both correctness-of-recall and shift measures. For a variety of reasons — semantic congruence of constituents, salience or imageability of the sentence-meanings as wholes, etc. — some contents may be relatively easy or relatively hard to cognize. Pitting each surface form against the other serves to control for such irrelevant factors. Table VI presents such data for the 16 sentences, along with the content words of each (in SVDO order to avoid the 'to' problem). Here 'correct' means perfectly recalled in the form given, 'shift' means recalled but in the form alternate to that given, and '0' means that either one or both (usually both) object NPs were forgotten. At the bottom of the table we record the summary for all 16 sentences: it is clear that when the SVOD form is presented, the sentence is more likely to be perfectly recalled as given, by a ratio of 2/1 over SVDO presentation; conversely, when the SVDO form is presented, the form in recall is more likely to be shifted, again by a ratio of about 2/1 over SVOD presentation. Both of these distributions are at the borderline of the .05 level of significance. We have inspected the deviant sentences — *the janitor gave a bath to his nephew* (both measures), *the secretary showed a dance to the professor* (both), *the boy showed a robin to his aunt* (both), *the farmer sold some books to the soldier* (correctness), *the florist gave a cat to the children* (correctness), and *the teacher threw a towel to the cook* (shifts) — but we cannot come up with any obvious reason why these sentences should 'prefer' the SVDO form.

TABLE VI

PERFECT RECALLS AND FORM-SHIFTS FOR THE SAME 16 BASE SENTENCES PRESENTED IN SVOD VS. SVDO FORMS ACROSS 17 SUBJECTS

Sentences				*Presentation Form*					
S	V	D	O	SVOD			SVDO		
				Correct	0*	*Shift*	*Correct*	0	*Shift*
janitor	gave	nephew	bath	1	1	7	8	0	0**
secretary	showed	professor	dance	2	1	5	3	6	0
librarian	loaned	policeman	clock	4	4	1	3	1	4
queen	made	gardener	five shirts	5	1	2	2	2	5
farmer	sold	soldier	some books	2	6	1	3	3	2
director	gave	coach	keys	5	3	0	1	7	1
customer	showed	grocer	map	3	5	1	2	3	3
teacher	threw	cook	towel	3	3	2	3	6	0
ambassador	got	accountant	letter	6	3	0	3	5	0
florist	gave	children	cat	1	6	2	4	2	2
boy	showed	aunt	robin	2	5	2	4	3	1
clown	sent	doctor	records	7	1	0	2	6	1
writer	rented	dentist	studio	7	1	0	2	3	4
agency	gave	family	nurse	6	3	0	3	3	2
landlord	showed	engineer	actress	7	1	0	4	5	0
store	awarded	mayor	one turkey	7	1	1	5	1	2

	More for SVOD	EQUAL	*More for* SVDO
Recalled as Given:	10	1	5
Shifted in Recall:	4	3	9

* The subjects coded "0" were ones who forgot one or both object NPs, usually both.

** Because of the odd number (17) of subjects, half of the sentences have 8 for SVOD and 9 for SVDO and half vice-versa.

Experiment III[25]

The third experimental study in this series utilizes a probe technique, borrowed from Weisberg (1971) but with some rather major modifications to be indicated. It is a type of word association procedure, which Weisberg calls Intrasentence Word Association (ISWA), where — presented a single sentence or a set of sentences — the subject is given key content words from the sentence(s) and instructed to respond with the first *other* word from the same sentence in which it occurred. As Weisberg notes (and as is characteristic of word association data generally), content words as cues usually yield other content words as responses and not function words, even though the latter may be closest in the sentence (e.g., given the cue word *built* from the sentence *sturdy houses are built by careful workers*, subjects very rarely produce *by*, but rather *workers, houses*, etc.). The general predictions here for us are (a) that V as a probe or cue will be more likely to elicit O than D as a response and (b), reciprocally, that as cues, O will be more likely than D to elicit V as a response.

The "tightness of cognitive binding" principle (V + O being parts of the relation component and D being the separate M_2 component) on which these predictions are based will necessarily interact with a 'left-to-right (forward) association' principle familiar in psychological studies of verbal learning — namely, that since sentences are normally processed in time from 'left-to-right' as they are spoken or written (even if the materials are presented visually), forward associations (leftward word as cue to rightward word as response) will be stronger than backward association (rightward word as cue to leftward word as response). Closeness in time-space will also increase associative tendencies. Such tendencies show up clearly in Weisberg's data. These factors relate directly to the alternative forms of the bitransitive that are presented: first, when V is the cue and O vs. D are the responses being compared, forwardness of association favors both O and D as responses, but in the SVOD form closeness favors O and in the SVDO form closeness favors D; second, when O vs. D as cues for eliciting V are being compared, backwardness of association works against both O and D, but closeness to V favors O in the SVOD form and D in the SVDO form. And there is one further complication — in the SVOD form the forward association with D will compete with the V response to O, whereas in the SVDO form the forward association with O will compete with the V response to D. We should expect the 'tightness of cognitive binding' factor to produce biases in the expectations based on purely directional and closeness associative factors.

Subjects. The participants in this experiment were also drawn from the psychology subject-pool. The initial design called for two groups of 10 subjects each, to be met in groups of five. Actually, 11 showed up for Group I and 8 for Group II — a total of 19. Data collection is continuing at the time of this writing with additional subjects.

Materials. The bitransitive and prepositional sentences were the same as used in Experiments I and II, but with the full 18 bitransitives and only 9 prepositionals to balance out the design. As a check on the effects of our modifications of the Weisberg (1971) procedures, we also included 9 of his sentences — either active or passive versions of what could properly be called 'generic'-type sentences (e.g.,

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[25] This experiment was planned jointly by Osgood and Schultze, and then conducted by the latter.

careful workers build sturdy houses, loyal nations are led by powerful kings, big men marry quiet women, exciting stories are told by skilled actors).

Procedure. Weisberg, working with individual subjects, presented a single sentence at a time, this sentence being read twice out loud by the subject and then immediately being probed with all content words from that sentence in random order. In our judgment, this task was more an intellectual exercise on the part of the subject, based on verbatim memory, than a spontaneous intrasentence word association task, based on recall of the meaning of the sentence as a whole. The procedure was therefore modified in the following ways:

(1) Sentences were shown *in sets of four*, each set on a slide and containing (in random order across slides) one bitransitive in SVOD form, another in SVDO form, one prepositional sentence, and one of Weisberg's 'generic' sentences in either active or passive form. Here we will only be concerned with the results for the bitransitives. Each subject group received nine such slides, thereby being exposed to 18 bitransitives (nine in each form), 9 prepositionals, and 9 'generics' — a total of 36 sentences. The nine bitransitives which were SVOD for Group I were SVDO for Group II, of course, and vice versa for the other nine.

(2) When each slide appeared, the *experimenter* read the four sentences in order at a natural pace, pausing between each, and the subjects were instructed to follow him with simultaneous silent reading. In the prior instructions, the subjects were told to try to get the *meaning* of each sentence as a whole (that this would help them in the task), even using imagery as an aid.

(3) Immediately after the reading of the four sentences on a given slide, the subjects turned their response sheets over to the new page, each sheet having 16 numbered blanks. The experimenter then gave 16 cue words drawn from the content words (four from each sentence) on the preceding slide, the orders of these cues with respect to the sentences being randomized across slides. The following are two illustrations:

(1) (5) (12) (7)
SLIDE 1: The janitor gave his nephew a bath.
(6) (9) (14) (2)
Cold bread is eaten by slow children.
(8) (16) (11) (13)
The dancer put the flowers on the trunk.
(10) (3) (15) (4)
The landlord showed an actress to the engineer.

(14) (6) (2) (12)
SLIDE 2: The detective chased the Martian with black spots.
(5) (11) (9) (13)
The secretary showed the professor the dance.
(16) (3) (1) (8)
The agency gave a nurse to the family.
(15) (10) (4) (7)
Trained artists paint correct letters.

with the orders of the cues indicated by the parenthetical numbers. The cues were given by the experimenter with just enough pause in between to allow the writing of a single word; subjects were instructed *not* to go back up the page and add in words that they had originally left as blanks (and they were closely monitored by the experimenter on this).

Results. We may first look at evidence for the purely associative factors discussed above — directional (forward stronger than backward associations) and temporal/spatial propinquity (closeness yielding stronger associations than remoteness in surface structures). Table VII gives the mean percentages of each type of response (including errors) to each type of probe or cue, (A) for both surface forms of the bitransitive combined, (B) when the basic SVOD form was given, and (C) when the transform SVDO was given. Forwardness of associations should facilitate both V → O and V → D and backwardness of associations should damp both O → V and D → V; in Table VII(A) we note that the former 'forwards' have values of 35% and 16%, while the latter 'backwards' have 16.5% and 9.5%, respectively. With the SVOD form given, closeness favors V → O over V → D, and we get 38% to 14%; with the SVDO form given, closeness favors V → D over V → O, but we get 32% V → O to 18% V → D — somewhat less of a difference due to closeness of D, but still with V more likely to elicit O as the postulated cognitive structure predicts. Similarly, closeness would predict O → V greater than D →

TABLE VII
MEAN PERCENTS OF TYPES OF RESPONSES TO TYPES OF CUES

(A) *For SVOD and SVDO Forms Combined*

		RESPONSES				
		S	V	O	D	ERRORS
CUES	S	—	24	24.5	22	29.5
	V	19.5	—	35	16	29.5
	O	30	16.5	—	32	21
	D	25	9.5	42.5	—	23

(B) *For SVOD Form Given*

		RESPONSES				
		S	V	O	D	ERRORS
CUES	S	—	26	22	20	31
	V	18	—	38	14	30
	O	26	18	—	38	17
	D	25	14	38	—	23

(C) *For SVDO Form Given*

		RESPONSES				
		S	V	O	D	ERRORS
CUES	S	—	22	27	24	27
	V	21	—	32	18	29
	O	34	15	—	26	25
	D	25	5	47	—	23

V when the SVOD form is presented (and we get 18% to 14%) and D → V greater than O → V when the SVDO form is presented (but we get only 5% D → V as against 15% O → V, again as expected from our theory of cognizing bitransitives). It was also expected that the forward association of O → D would compete with the backward association of O → V when SVOD is given (and we get 38% vs. 18%, suggesting a strong associative effect here); similarly, when SVDO is given, we would expect forward D → O to compete with backward D → V (and here this effect is much greater, 47% D → O vs. only 5% D → V, again testifying to the looser cognitive binding of D with V as predicted by the theory).[26]

It is clear in the evidence above that the postulated binding of O with V as against the independence of D (as the M_2) from V is creating biases in the expected directions from what would be predicted from purely associative factors. Since the percentages in Table VII do not lend themselves to tests of statistical significance, we have applied sign tests (for the 18 sentences across subjects) of the major predictions.

Prediction (a) was *that V as a cue will be more likely to elicit O than D as a response, regardless of the form of bitransitive given*; but if we do take the form given into account — since the V + O relation is broken up in the SVDO form — we must add *that the difference between V → O and V → D will be greater when SVOD is given than when SVDO is given*. For the SVOD form, we find that 15 sentences support this prediction and only 3 go against it (a distribution that is significant at the .01 level); for the SVDO form, the distribution is 10 in favor, 3 neutral, and 5 against (in the right direction, but not significantly so); so both facets of Prediction (a) are supported.

Prediction (b) was *that O as a cue will be more likely than D as a cue to produce V as a response, regardless of the form given*; but if we again take into account the form given, then — since O is closer to V in the basic form of bitransitive — we must add *that the difference between O → V and D → V will be greater when SVOD is given than when SVDO is given*. Here, although the overall prediction is borne out, the form-specific prediction is neatly reversed; with SVOD given, only 5 sentences support the prediction, 9 are neutral (equal percentages), and 4 go against, whereas with SVDO given, 11 sentences support, 7 are neutral, and 0 go against — the latter distribution being significant at the .01 level. Why this reversal? Since both O → V and D → V are backward associations, with O → D (in the SVOD form) and D → O (in the SVDO form) being competing forward associations, we might look here for the answer. It must be predicted that O → D will be greater for SVOD than for SVDO (and this holds up, but with only a 12/6 ratio, which misses the .05 level) and that D → O will be greater for SVDO than for SVOD (which yields a 14/4 ratio, which is significant at the .025 level). Now looking back at Table VII, we note that there is a general avoidance of V as a response to D as a cue *and this is particularly the case* (only 5%) *when the SVDO form is given* where purely associative factors (closeness) should favor the D → V connection. This suggests that the insertion of D before O in the surface structure, thus interrupting completion of the relation (-- (O) -- >), does require temporary displacement of the dative NP information to the BUFFER — and it is *this* that effectively 'insulates' V from D as an associate.

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[26] It might also have been expected from these association principles that the closeness of S to V as compared with the relative remoteness of O and D from S in both forms would yield much greater S → V than other responses; this is clearly not the case, and the reason is another familiar 'law' of word association — that the form-class of the stimulus word (noun) produces the same form-class as a response!

General discussion

Summary of evidence bearing on the theory proposed

We proposed as the *Naturalness Principle* that the more surface structures of sentences correspond in their ordering of semantic information to that characteristic of pre-linguistic, perceptual cognizing of events, the more easily (other things being equal) will they be understood and produced. Evidence was offered that increasing the number of simple cognitions (clauses) produces more processing difficulty than increasing the complexity of the constituents of simple cognitions — suggesting that languages should tend diachronically to develop means of packing more information into simplexes rather than elaborating semantically equivalent complexes. Cognitions involving three entities thus put pressure on a theory that postulates an underlying tripartite [M_1 – – (M) – –> M_2] structure for simple cognitions — either to 'absorb' the third entity into one of the basic components or to demonstrate that a conjoining of two simplexes into a complex is involved. Since the prototype bitransitive involves transferring an inanimate entity from one human Source to another human Recipient, in theory

M_1 – – (M) – – > M_2
[PERSON A / TRANSFERS OBJECT X TO / PERSON B],

this type of sentencing provides an ideal situation for testing the notion that the third entity (here, the DO) can be 'absorbed' into the relation as part of it (the embedded M).

Cross-language linguistic evidence. Basing our analysis primarily on papers by Blansitt (1973) and Sedlak (1975), the latter designed as an explicit testing of "the Osgood Hypothesis", we made a number of predictions: Predictions #1 all concerned ordering of D vs. O with respect to V: the overall prediction, (a) that, regardless of language type or marking characteristics, O will be closer to V than D, was *not* upheld (and the semantic transparency of prototypic bitransitives as well as competing tendencies were offered as explanations); based on the notion of BUFFER storage-time, the prediction (b) that the tendency for O to be closest to V would be least for SVO and most for VSO, was clearly upheld. Predictions #2 all concerned marking characteristics: the prediction (a) that D will be more frequently marked than O, was supported by a consistent ratio of 2:1; (b), that D-marking should be relatively greater for 'unnatural' transforms than for 'natural' basic forms in the theory, was supported 10-to-4 vs. 7-to-6 languages for transforms and basics, respectively); (c), that marking of D should be greater for nonprototype than for prototype bitransitives, was supported by a few languages described by Blansitt.

Prediction #3 — that if O is marked in unitransitive but not in bitransitive clauses, then the same marker will be used for D in bitransitives — was upheld by a surprising number of languages cited by Blansitt (13 spread across all types); this, being a case where the 'real' DO stands out on the surface, was the inspiration for the title of this paper. Prediction #4 — that O is more likely to be incorporated in verbs than D — was strongly upheld in both American Indian data reported by Sapir (1911) and data on Mandarin Chinese reported by Chao (1968). And finally, Prediction #5 — that the presumed basic form will be less constrained semantically and syntactically than the transform — was clearly upheld in American English data and (in a special study by the second author) in data collected on pronoun D vs. O constraints for 13 SVO languages, but the expected difference did not appear in the data for 12 SOV languages.

Experimental psycholinguistic evidence. Three experiments were designed jointly by Osgood, Tanz, and Schultze, all testing in various ways the basic notion that DO is more tightly bound to V than is IO. Experiment I tested the specific hypothesis that, given recall of the verb, the conditional probability of also recalling DO will be greater than that of recalling IO; as predicted, there were significantly fewer forgettings (.003 level) and fewer changings (.05 level) for DO as compared with IO — and there were no differences in recalling or changing the two objects when V was *not* recalled (although the number of cases is very small). Experiment II, with both subject and verb given as cues for recall, tested two specific hypotheses: (1), that overall DO will be better recalled than IO, regardless of form given, yielded a null result, the reason being that subjects under these conditions nearly always either got both object NPs or neither; (2), that with SVOD presented there will be more correct sentence recalls and fewer shifts from the surface form given than with SVDO presented, was supported in both respects (just missing the .05 level of significance in each case for sentences). Experiment III, using words from sentences as cues for recalling other words in the sentences, tested two specific hypotheses: (1), that V as a cue will be more likely to elicit DO than IO, with this tendency being greater for SVOD than for SVDO, yielded positive results significant at the .01 level; (2), that as cues DO will be more likely than IO to elicit V, and again with this tendency being greater for SVOD than SVDO, was confirmed for overall elicitation of V but the form-specific prediction was reversed — for a possible reason that is discussed.

Given the relatively small number of cases involved — of languages (when broken down in terms of basic types, fixed vs. variable orderings, and marking characteristics) and of both experimental subjects and sentences in the case of the psycholinguistic experiments (the *N* of subjects is being increased) — the highly consistent trends in favor of the hypotheses (with only a few exceptions) across both the linguistic and psycholinguistic analyses is gratifying. Nevertheless, we would like to see both types of analyses extended.

In the remainder of this general discussion we will first take up two diachronic matters of theoretical interest — language development in children (which *is* a diachronic process on a small time scale, but for individuals rather than languages) and the development of languages (on a much grander time scale), both in relation to cognizing bitransitives. Then we will consider briefly the intriguing problem of deixis as it appears in bitransitives (directions *away from* vs. *toward* the subject of the sentence).

Bitransitives in the development of language in children

Research on child language acquisition tends to support the proposition that, in English, the [S – VO *to* – D] form of the bitransitive is more basic than the [S – V – D – O] form. In her doctoral thesis, Fischer (1971) studied imitation and comprehension of three forms of the bitransitive,

(1) S V O *to* D (The X gave the Y to the Z)
(2) S V D O (The X gave the Z the Y)
(3) S V *to* D O (The X gave to the Z the Y),

where (3) is of questionable grammaticality in adult English (?*The judge gave to the defendant a stiff sentence*), albeit perfectly comprehensible. One-third of the sentences given the children (3;3-4;3 in age) were of each type. In both imitation and comprehension tasks, the children performed much better on Type (1) sentences (which we consider closest to the pre-linguistically based cognitive structure) than on Type (2). Fischer also notes (1971:104) that ". . . in the 'talking about the other picture' task the children almost invariably used [*the Type (1)*] structure", i.e., when Simply Describing. She also reports (1971:105-108) that semantically plausible sentences (or better, sentences plausible in terms of knowledge of the world) that are reversible (e.g., *the farmer is feeding the snake the cookie*) had 76% accuracy of response as compared with implausible reversible sentences (e.g., *the farmer is feeding the cookie the snake*) which had only 34% accuracy.

Waryas and Stremel (1974)[27] report an unpublished paper by Stayton (1972) in which 10 children (2;10-4;4 in age) were given "commands"

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[27] The Waryas and Stremel (1974) study itself, using preference ratings by adult subjects, confirmed some of the major hypotheses tested in the present research, but using highly nonprototypical sentences with all ⁺Human NPs (*the boy gives the girl to the man*) or PNs (*the boy*

(apparently like, *give the bear the dog, give the box to the marble*, since it is stated that DO and IO were either *both* $^+$Animate or $^-$Animate in order to eliminate purely semantic cues) in nine different word orderings, including ungrammatical and ambiguous ones. Stayton reports that these rather young children, in a very difficult task, performed more accurately in the Simply Acting Out situation, both when the first noun mentioned was the DO and when the *to* was present as an indicator of the IO; 95% or more responded correctly to the V – DO – *to* – IO as against only 30-45% for the V – IO – DO sentences. In a very recent Illinois study, Thieman (1975) reports that in cued recall his youngest subject (age 3;8) recalled both Type 1 and Type 2 sentences in the Type 1 form (given *the teacher read her children a story* by the experimenter this child came back with *the teacher read the story to the children*); similarly, in the benefactive type of bitransitive, the shift was also to the "basic" form (E: *the girl's father made her a dollhouse*; C: *the man built a dollhouse for the girl*). Worth noting is the fact that *in an imitation task* this same child was able to repeat the same sentences verbatim.

Although these data from English-speaking children are consistent with the theory, they are not conclusive. In English, word order and marking vary simultaneously in the two forms of the bitransitive, and one cannot therefore rule out the possibility that it is the presence of *to* which makes Type 1 sentences easier than Type 2. It will be recalled that Fischer (1971) included as a Type 3 the S V *to* D O form: although the older subjects (3;9-4;4) did somewhat better in comprehending this form than the unmarked Type 2 (but with p = only < .10), the younger subjects (3;3-3;8) in both comprehending and imitating and the older on imitating generally did more poorly on Type 3 sentences than the other types. But, of course, Type 3 is of questionable grammaticality in adult English.

What about the handling of bitransitives by children in other languages where the confounding of order and marking is not present? McNeill, Yukawa, and McNeill (1971) provide us with just such a case—Japanese, which is an SOV-type language in which DO and IO *must* be marked by postpositions and order for adults is free (thus *the turtle-ni* (IO) *the fish-o* (DO) *push* and *the fish-o* (DO) *the turtle-ni* (IO) *push* are equally acceptable and both mean 'push the fish to the turtle'). Question: Is word-order equally free for children learning Japanese as their native language? Using about 10 children in each of three age groups (2;3-3;1, 3;7-4;6, and 5;0-5;7) and imperatives like the above to be Simply Acted Out (but again

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gave her to him): when both objects were nouns or both were pronouns, the DO-*to*-IO form was consistently preferred over the IO-DO form; when IO is nominal and DO pronominal, the DO-*to*-IO form is preferred, but when the reverse is the case, although the 'basic' form is still preferred, the differences are greatly reduced.

with the semantic control that *both* DO and IO in any sentence had to be either $^{+}$Animate or $^{-}$Animate), these investigators found that, although performance accuracy generally increased with age, *forms with DO closer to V than IO (and particularly with DO preceding V as is "natural" for an SOV language) quite consistently were handled more correctly than forms with IO closer to V*. The most critical comparison is between the fully grammatical and equally likely forms (for adults) where both objects are differentially marked and the SOV basic structure is kept:[28] the percentages correct across the three age groups are, for IO-ni DO-o V, 80%, 82%, 100%; the corresponding values for DO-o IO-ni V are 40%, 73% and 70%. This is a rather striking difference and it is consistent with our theoretical expectation that — since DO + V are part of the same cognitive component — sentences with DO close to V should be easier for children to process than those with IO closest to V.

We are presently piloting with American English-speaking children a study on the acquisition of bitransitives which we plan to replicate in as many of the 30 language-culture communities involved in our cross-cultural project on affective meanings (see Osgood, May and Miron 1975 for details) as colleague interest and sophistication in research with children make feasible. Using both comprehension (Simply Acting Out) and production (Simply Describing) measures, we are studying (1) word-order effects and (2) reliance on the animateness of IO and the inanimateness of the DO by the children.[29] A parallel study is soon to get underway with Persian in Iran,[30] which is also a language in which bitransitive orders vary but without varying grammatical marking of the objects; however, in Persian (in contrast to Japanese) it is only the IO that is marked — the two alternative orders thus being S O to-D V or S to-D O V. Our prediction is that the latter form will develop earlier than the former, despite the fact that our informant (Dr. Farideh Hoosain) feels that the former is more natural for adult speakers. Confirmation of the hypothesis would be strongly confirmatory of the general theory under these conditions.

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[28] Many other conditions, all ungrammatical (or at least of questionable grammaticality) were included in this study — with only DO or only IO marked or with V in either first or middle position in the sentences (rather than SOV in form) — but they are not directly relevant to our interests here.

[29] This research is being conducted by Annette Zehler at the Children's Research Center (the Institute for Child Behavior and Development) at the University of Illinois; the Center's assistance is gratefully acknowledged.

[30] This research in Iran is being conducted by Dr. Farideh Hoosain, at Feradowsi University.

Development of bitransitive structures in Languages

Blansitt (1973:6) reports that, with only a couple of exceptions, deletion of the D in the bitransitive ordering leaves the basic unitransitive order type for that language (see columns 1, 2, and 3 below). The two exceptions are GUMBAINGAR and MENDE, which appear to shift to an SVO type with dative deletion (that is, from SOV unitransitive to SD̸VO in bitransitive). The existence of exceptions to this "rule" demonstrates that it is not an automatic artifact; the 'rule' itself is what one might expect if the D were inserted into an already established basic unitransitive type later in language evolution. However, there is something else to be noted here: if we alternatively delete the O from the bitransitive form and — in keeping with our theory of the matter — view the so-called D as being the 'real' O, then, as shown in the last column, such deletion also restores the basic unitransitive type, as if the 'absorption' of the O into the V relation leaves the basic cognizing order. In this case, the two languages which shifted basic order-type with D-deletion are restored to normalcy, but the O-deletion, with D viewed as the 'real' DO, also fails for one bitransitive type which includes three languages (HUICHOL, KALKATUNGU, and TARAHUMARA).

Bitransitive order	*D Deletion*	*Unitransitive order*	*O Deletion*
SVDO	SVD̸O	SVO	SVDØ
SVOD	SVOD̸	SVO	SVØD
SDOV	SD̸OV	SOV	SDØV
SODV	SOD̸V	SOV	SØDV
SOVD	SOVD̸	SOV	X SØVD
SDVO	X SD̸VO	SOV	SDVØ
VSOD	VSOD̸	VSO	VSØD
VSDO	VSD̸O	VSO	VSDØ
VODS	VOD̸S	VOS	VØDS

It seems reasonable to assume that languages have evolved in complexity from two-entity to three-entity simple cognitions, where both engage only a single relation (VP). The fact that both D-deletion and O-deletion in bitransitives (above) are successful in restoring basic typologies suggests two possible lines of such language evolution: (1) insertion of the third entity into the existing simple cognition, thus [M_1 ------> M_3 M_2] where M_3 is the D, or (2) absorption of the third entity into the relation, thus [M_1 -- (M_3) -- > M_2] where M_3 is the O and M_2 is the Recipient (the 'real' direct object) — the latter being the alternative espoused here, of course. With regard to these mental perambulations, Greenberg (in his 6/21/74 letter to C.E.O.) had the following to say:

> You talk about D being inserted "later in language evolution". But, of course, all known languages have these constructions and there is no empirical way to discover the nature of such earlier structures. . . . But it does bring up perfectly researchable problems if looked at another way . . . namely, in terms of diachronic universals principles . . . it must be that whenever a language changes its basic order, the bitransitive construction must keep pace by a corresponding change. Hence your "later in language evolution" becomes "dependent variable in change".

Since Greenberg didn't refer us to any diachronic studies of this sort on the development of bitransitives, we assume that none existed.

Devoting the last section of his paper to *serial verb constructions in bitransitives*, Sedlak (1975:141-147) has taken a step toward filling this gap. Although this construction occurs in a limited number of languages of the world (NIGER-KORDO-FANIAN languages of West Africa, in AKAN and some other KWA languages), the implications for language evolution are not limited. Sedlak also notes that "in some of these languages, serial verbs have been calcified as 'prepositional' markers" (1975:141). In bitransitives serial construction has the form *take X give Y* in most of these languages, where X is the DO and Y is the IO; other verbs of transfer may substitute for *give*, thus *take X* ⟨*lend, sell, offer*⟩ (give) *Y*, where, as indicated by the parenthetical (give), the prototypic verb *give* may also appear next to *Y* (the Recipient), now as a kind of marker. Sedlak makes the insightful remark (1975:141) that "serial verbs are contained within a single sentence *in which a succession of acts is performed by the same person*, the subject-actor" (italics added).

Let us now explore this as an instance of a more general phenomenon of fusing double conjoined cognitions (complexes) into single cognitions (simplexes).[31] First, take the case where only the relations (VPs) are variables:

M_1 – – (M) – –$>_1$ M_2 and M_1 – – (M) – –$>_2$ M_2
[Adam shelled the peanuts] then [Adam ate the peanuts];

fusing the different relations we would get

M_1 – – (M) – –> M_2
[Adam shelled-and-ate the peanuts],

which via redundancy deletion, the "shelling" being presupposable, yields

[Adam ate the peanuts].

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[31] We do this only for SVO-type languages (and here for English, of course), but the same type of analysis presumably could be made for the other types; it is also likely that, not being trained linguists, our way of presenting this hypothetical evolution is not as elegant as it might be!

Take next the case in which, for the same Actor, both relations and objects are variable:

$M_1 - - (M) - - >_1 M_2$ and $M_1 - - (M) - - >_2 M_3$
[Adam grasped the apple] then [Adam cut (it) with a knife];

again fusing relations and here also the different objects we would get

$M_1 \quad - - (M) - - > \quad M_3$
[Adam grasped-apple-and-cut-apple with a knife],

which, via redundancy deletion, the prior "grasping" being presupposable, yields

[Adam cut the apple with a knife].

And take, finally, the bitransitive case in which there is only one Actor and one relation, but two objects:

$M_1 - - (M) - - >_1 M_2$ and $M_1 - - (M_2) - - >_2 M_3$
[Eve took the apple] then [Eve gave (it) Adam];

again fusing these cogs into a simplex, we get

$M_1 \quad - - (M_2) - - > \quad M_3$
[Eve took-apple-gave-apple Adam],

which, with redundancy deletion of the first relation, being presupposable, yields

[Eve gave the apple (to) Adam].

The general principle suggested here is this: when the variable components in two conjoined cogs having a common subject are redundantly ordered in time (here, "shelling" before "eating," "grasping" before "cutting," and "taking" before "giving"), it is the elements in the prior cog that are presupposable and hence deleted. In an investigation of Simply Describing (i.e., paraphrasing) perceived events with sentences, the first author (Osgood 1971) repeatedly observed this phenomenon; for example, the first perception of A SMALL BLACK BALL ROLLS AND HITS A BLUE BALL would frequently be described with conjoined clauses as *rolls and hits*, but a second related perception of THE SMALL BLACK BALL ROLLS AND HITS A BIG ORANGE BALL would usually drop the first clause, yielding *the black ball hit a big orange ball.*

Sedlak (1975:145-147) also presents evidence for MANDARIN and other Chinese languages *that basic transfer verbs may also function as dative markers in bitransitives*, and he notes further that in the latter case they appear to be a sub-type of the serial verb. Thus we have *gei* ('give') as a verb in the unitransitive *Wo gei ta tang* ('I *give* him candy') but *gei* ('for') as a dative marker in the benefactive bitransitive *Zhang-san gei wo mai yifu* ('Zhang-san *for* me buy clothes'). In cases like this we seem to have 'prepositions' directly derived from transfer verbs that have been 'demoted' (reduced to a marker function). Sedlak concludes his section on serial verbs as follows: "In the case of both languages of West Africa and a number of CHINESE 'dialects', the following historical development has been observed for serial verbs. If a language of this type develops a prepositional marker for the indirect object, this marker is derived from a calcified form of the verb 'give'. The direct object has no such marker" (1975:146-147). Thus we have an evolutionary sequence: from serial verbs (indicating the fusion of redundantly conjoined cognitions) to the same verb stems being used as dative markers — and an interesting question, of course, is whether it would be possible to show that dative markers generally (like the *to* in English) are traceable diachronically to calcified transfer verbs.

At the end of the section of this paper on Theoretical Background (p. 550) we suggested that the critical difference between prepositional and bitransitive three-entity sentences (in English, at least) — the former requiring retention of the preposition in phrase reversals (*Khrushchev banged on the table his shoe* / **Khrushchev banged the table his shoe*) but the latter involving deletion of the preposition for naturalness (*Khrushchev gave the chairman his shoe* / *?Khrushchev gave to the chairman his shoe*) — raised questions about the status of the *to* in bitransitives *as a preposition*. Evidence for the evolutionary development of dative markers from prototypic *give* verbs would seem to heighten the seriousness of this question. In the same section (pp. 547-548) we demonstrated that application of the passive transformation consistently distinguished 'true' prepositional phrases (which had to be transformed with the direct objects as wholes (e.g., *the old lady across the street was helped by the gardener*) from 'prepositions' that are functionally parts of the verb phrases (and therefore can be separated from the direct objects in the transformation, e.g., the entirely different meaning conveyed by *the old lady was helped across the street by the gardener*).

It would appear that the *to* in bitransitives is also 'verb-absorbed'; applying the same passivization test, note the strangeness of **the book to Mary was given by John* as compared with the naturalness of *the book was given to Mary by John*. We also noted earlier in the cross-language linguistic evidence (pp. 555-556) that SVO variable-order languages tend to drop the extraverbal prepositional marker in the transform and SVO fixed-order languages are nearly always intra- rather than extraverbal marking, thus mainly without prepositions (the overall ratio in Sedlak's data for both variable and fixed SVO being 16 of 20). The loss in transformation (variable languages) or lack of extraverbal marking (fixed languages) of *to*-equivalents may well be due to the fact that the underlying relation (– – (DO) – –>) must be, e.g., *gave the book to* (not *gave to the book*), and hence the strangeness of *John gave to Mary the book* — because the *to* in *gave* (DO) *to* is part of the VP and not a 'true' PP.

Deixis in the bitransitive mode

The direction of transfers expressed in bitransitive sentences, *away from* vs. *toward* the Actor-subject, poses intriguing problems. This semantic distinction is at least a quasi-deictic one. Deixis usually has to do with direction or position in relation to the speaker and typically involves prepositional phrases (e.g., *the ball is in front of the cup*) but may also have an entity that possesses a 'front' and a 'back' as the reference point (e.g., *the man is in front of the car*). In the bitransitive case the reference point is not the speaker of the sentence but rather the Actor-subject of the sentence — thus prototypical *give* as in *John gave the bread to Luke* involves transfer of the inanimate object *away from* the Actor to the Recipient whereas non-prototypical *take* as in *Matthew took the wine from Mark* involves transfer of the inanimate object *toward* the Actor from the 'Recipient' (as do other verbs like *borrow, buy, rent*, etc.). It is clear that the *toward*-Actor verbs have quite different (more constrained) syntactic properties than the *away-from* Actor verbs, but the psycholinguistic (cognitive) reason for this is most unclear.

The prototypic *away-from* verbs have two passives — from *John gave the bread to Luke* we can transform into both *the bread was given to Luke by John* and *Luke was given the bread by John*, the latter fronting the dative; *toward* verbs have only one passive — from *Matthew took the wine from Mark* we can transform into *the wine was taken from Mark by Matthew* but not into *Mark was taken the wine by Matthew* (where the meaning changes) or **Mark was taken the wine from by Matthew*, the fronting of the dative thus being blocked. It should be noted that the object phrase reversibility that is the hallmark of prototypic bitransitives in languages with variable order (*the monkey gave the hat to the bear* / *the monkey gave the bear the hat*) does not hold for nonprototypic *toward*-Actor verbs (*the monkey took the hat from the bear* / *the monkey took the bear the hat*), since the meaning completely reverses along with the phrase reversal — from *toward*-Actor into *away*-from Actor.

From the perspective of case grammar, verbs like *take* in bitransitives are quite puzzling in that they seem to straddle case assignments and therefore call into question existing case categories. In a sentence like *Matthew took the wine from Mark*, perceptually MATTHEW would be perceived as the initiator of the action but also as the Recipient of the WINE. Therefore, *Matthew* in the sentence would be Agentive and yet also partake in something of the Dative case. Now *this* may have some interesting psycholinguistic implications: is it possible that, in pre-linguistic cognizing of such *toward*-Actor transfer events, *the 'Recipient' loses his prototypic* $^{+}$*Animacy and, in effect, the relation does become more prepositional than bitransitive*? In perceiving DADDY TOOK THE HAT FROM THE DOG, if the animal has an entirely passive role, this event may be cognized as would DADDY TOOK THE HAT FROM THE RACK.

Applying the same reasoning to the sentence *Matthew took the wine from Mark*, the passivization (de-Animating) of the 'Recipient' should shift the IO (Dative) to a prepositional O (Ablative). And this is what seems to occur. Note that just as *Matthew took the wine from Mark* cannot be passivized as **Mark was taken the wine from by Matthew*, so too can the prepositional *Matthew took the wine from the cellar* not be passivized as **the cellar was taken the wine from by Matthew*. And note also that, just as *the old lady across the street* is ambiguous under the passivization test (*the old lady across the street was helped by the gardener* has a different meaning than *the old lady was helped across the street by the gardener*), so too is the *toward*-Actor bitransitive ambiguous (*the wine from Mark was taken by Matthew* — thus "*Mark's wine*" — vs. *the wine was taken from Mark by Matthew*) and so also is the clear locative prepositional (*the wine from the cellar was taken by Matthew* — thus the "cellar wine", not the ordinary wine in the cupboard — vs. *the wine was taken from the cellar by Matthew*).

This psycholinguistic analysis obviously implies that such deictic effects of *away-from* vs. *toward* Actor transfers should be universals of human language. Although neither Blansitt (1973) nor Sedlak (1975) — presumably because of their concentration on prototypical *give*-type bitransitives — presents any data on this matter, we have come across a most intriguing case of a language where the predicted shift from dative (with *give*) to ablative (with *take*) stands right up in the surface structure in terms of overt marking.[32] In KANNADA, an SOV language of Southern India, semantic relations such as Recipient, Instrument, etc., are marked by discrete case-endings. Thus in the *give to* type of transfer,

> Matthew Mark-ge drākshārasavannu kottanu
> Matthew (nom.) Mark (dat.) wine (acc.) gave
> 'Matthew gave the wine to Mark'

the Recipient is marked by the dative ending *(-ge)*. In the *take from* type of transfer, however,

> Matthew Mark-inda drākshārasa vanu tegedukondanu
> Matthew (nom.) Mark (abl.) wine (acc.) took
> 'Matthew took the wine from Mark'

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[32] We thank S.N. Sridhar of our Center for Comparative Psycholinguistics staff (and a native speaker of Kannada) for bringing this to our attention.

the Source NP is marked with an ablative ending *(-inda)*. Note that the case marking is the same whether the Source NP is Animate (and Human, as here) or is Inanimate, as in

> Matthew nelamālige-inda drākshārasavannu tegedukondanu
> Matthew (nom.) cellar (abl.) wine (acc.) took
> 'Matthew took the wine from the cellar'

What is particularly interesting is the fact that this 'de-Animation' of the Source NP is also reflected in the case-marking of the 'demoted' subject NP in the passive transformation in Kannada, thus

> Matthew Mark-inda hodeyalpattanu
> Matthew (nom.) Mark (instr.) hit-passive-past
> 'Matthew was hit by Mark'

Although the case ending on the Source NPs in the *toward* Actor bitransitives and on the 'demoted' subject NP in the passive has been differentially translated as ablative and instrumental, the morpheme employed is formally identical — probably reflecting the underlying functional (psycholinguistic) similarity, according to our informant. It would be most interesting to check such deictic effects across a representative sample of human languages.

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On the Count of One

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0.1. Sentential second position is important cross-linguistically as the position in which a certain class of elements is found.[1] In languages as diverse as Tagalog, Walbiri, Papago, and Sahaptin, one or more of the following elements occur in sentential second position: subject marking, object marking, tense, aspect, modality, conjunction, emphatic particles, negation, and question marking.[2] In German the tensed part of the verb occurs in second position; in English sentential adverbs can occur in second position (as does the auxiliary). Wackernagel noted a general tendency in Indo-European for certain sentential particles and pronominal elements to occur in second position in the sentence. WHY sentential second position is important is a question that has received a little attention, although certainly not its due.[3] Most hypotheses suggest that the importance of sentential second position is a consequence of the importance of sentence initial position. Clause initial position is an obvious position for elements with sentential scope; the class of elements listed above, with the possible exception of subject and object marking, has sentential scope.[4] Hale (1973), in his discussion of the Walbiri auxiliary, argues that the auxiliary is to be generated clause initially and moved to second position; why the auxiliary is moved is left an open question. In Steele (1975), I argue that the occurrence of modality in sentential second position results from modals being moved out of sentence initial position by a subject or a topic.[5]

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[1] Mrs. Villiana Hyde is the source of the Luiseño data; everyone working on an exotic language should have the luck of finding someone as good as she is. Richard Oehrle lent me his ear and his mind for long stretches of time; much thanks is due to him and them. In addition, Wallace Chafe, Charles Fillmore, Kenneth Hale, Margaret Langdon, and Edith Moravcsik offered helpful and insightful comments at crucial times. Research for this paper was supported by the American Council of Learned Societies. This is a revised and expanded version of a paper given at the Fifth Annual California Linguistic Association Conference.

[2] Possessive pronouns can occur in sentential second position as well, although somewhat less frequently (cf. Tagalog and Pashto).

[3] The importance of second position is apparently something which it is difficult to develop an awareness of. Many grammars of languages where some of the elements listed occur in second position comment on the possibility that these elements can attach to anything in the sentence, never noticing that the anything has to be initial. Linguistic theoreticians who deal primarily with English data seem totally unaware of the cross-linguistic importance of sentential second position and how it might bear on their analyses of English (see Jackendoff's discussion of sentential adverbs).

This paper assumes the cross-linguistic importance of sentential second position; it examines how sentential second position is defined in one language. The discussion bears directly on the question of why sentential second position is important.

0.2. Luiseño, a Uto-Aztecan language of Southern California, has an auxiliary composed of clitic subject pronouns, tense clitics, and modality clitics.[6] An auxiliary in a declarative sentence contains at least a clitic subject pronoun; the presence of a particular tense clitic or a particular modality clitic depends on the time of the situation described in the sentence or the potentiality of the situation described in the sentence,

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[4] Clause final position is also an obvious position, but as I argue (Steele 1975), the beginning of the sentence is more highly valued for these elements than the end of the sentence.

[5] *Topic* was the term I used. It is a poorly chosen term. A better choice might have been *theme*, in Kuno's sense.

[6] By *clitic* I mean enclitic; the clitic complex is loosely attached to the immediately preceding element. Attachment may be necessary due to the unstressed — and unstressable — character of the elements of the auxiliary. In any case, attachment is evidenced by the operation of some word internal phonological processes. /š/ becomes /č/ before a vowel, word internally.

kiiča	’house’
kiiš	’house (object form)’

The addition of an auxiliary which begins with a vowel to a word which ends in /š/ will cause that segment to become /č/, at least under non-slow speech conditions.

yaˀaaš heelaq
man is:singing
‘The man is singing.’

yaˀaačup heelaq
yaˀaaš=up
man=AUX is:singing
‘The man is singing.’

I am using the term *auxiliary* to refer to a complex of clitic elements. Although the composition of the complex parallels in important respects the English auxiliary as analyzed in *Syntactic Structures* and some of the revisions thereto, the use of the term *auxiliary* as applied to the Luiseño clitic complex is not exactly analogous to the use of the term as applied to the English auxiliary. The Luiseño auxiliary is necessarily clitic; the English auxiliary contains elements which may be but are not necessarily. The Luiseño auxiliary contains verbal categories but the members of it are in no sense auxiliary verbs; the English auxiliary contains auxiliary verbs. The Luiseño auxiliary contains clitic pronouns; the status of subject agreement as a part of the English auxiliary is in doubt. The Luiseño auxiliary is a surface unit; the English auxiliary is not necessarily.

respectively.[7] The (full) auxiliary is illustrated in (1).

(1) modal-subject pronoun-tense

(2) and (3) contain example sentences. The first sentence in (2a), (2b), (2c), (3a), (3b), and (3c) is in the present tense; the auxiliary contains only a clitic subject pronoun. Note that the clitic pronoun agrees with the subject of the sentence.[8] The second sentence in each set of examples is in the past tense; the auxiliary contains a tense clitic (*il*) which matches the tense of the verb, as well as a clitic pronoun.[9] The third sentence in each set of examples is in the future tense; the auxiliary contains a future tense clitic (*po*) which matches the tense of the verb, as well as a clitic pronoun. The last sentence in each set of examples has a modal gloss; the auxiliary contains a modal clitic, a clitic pronoun, and a tense clitic.[10]

(2) a. noo n heyi-q
I I dig-present
I am digging

noo n=il heyi-quś
I I=past dig-past
I was digging.

noo nu=po heyi-n
I I=future dig-future
I will dig.

noo xu=n=po heyi
I modal=I=future dig
I should dig.

b. wunaalum pum heyi-wun
they they dig-present
They are digging.

wunaalum m=il heyi-quś
they they=past dig-past
They were digging.

---

[7] The auxiliary is not necessarily present, unless it contains modality. But, if it occurs, all the parts of it occur.

[8] The clitic pronouns in the present tense are:

n	ča
up	um
up	pum

The forms differ slightly in different tenses.

[9] There are a number of past tenses in Luiseño; *il* is the tense clitic for all of them.

[10] The following conventions are adopted for the example sentences: a hyphen (-) indicates a morpheme break within a word; an equal sign (=) indicates the division between the members of the auxiliary; a colon (:) is used in the English literal gloss when a Luiseño word

wunaalum mo heyi-n
they they:future dig-future
They will dig.[11]

wunaalum xu=m=po heyi
they modal=they=future dig
They should dig.

c. čaam ča heyi-wun
we we dig-present
We are digging.

čaam čam=il heyi-qus̸
we we=past dig-past
We were digging.

čaam ča=po heyi-n
we we=future dig-future
We will dig.

čaam xu=ča=po heyi
we modal=we=future dig
We should dig.[12]

(3) a. noo n hunwuti pati-q
I I bear(obj) shoot-present
I am shooting a bear.

noo n=il hunwuti pati-qus̸
I I=past bear(obj) shoot-past
I was shooting a bear.

noo nu=po hunwuti pati-n
I I=future bear(obj) shoot-future
I will shoot a bear.

noo xu=n=po hunwuti pati
I modal=I=future bear(obj) shoot
I should shoot a bear.

b. wunaalum pum hunwuti pati-wun
they they bear(obj) shoot-present
They are shooting a bear.

wunaalum m=il hunwuti pati-qus̸
they they=past bear(obj) shoot-past
They were shooting a bear.

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is glossed by an English sequence of two or more words. The auxiliary is written separate from the preceding word, but cf. footnote 6.

[11] *mo* is the realization of the subject marker *m* and the future clitic *po*

[12] The phonetic realization of xu=ča=po is xus̸po.

wunaalum mo hunwuti pati-n
they they:future bear(obj) shoot-future
They will shoot a bear.

wunaalum xu=m=po hunwuti pati
they modal=they=future bear(obj) shoot
They should shoot a bear.

c. čaam ča hunwuti pati-wun
we we bear(obj) shoot-present
We are shooting a bear.

čaam čam=il hunwuti pati-qus̸
we we=past bear(obj) shoot-past
We were shooting a bear.

čaam ča=po hunwuti pati-n
we we=future bear(obj) shoot-future
We will shoot a bear.

čaam xu=ča=po hunwuti pati
we modal=we=future bear(obj) shoot
We should shoot a bear.

(2) and (3) illustrate as well the position of the auxiliary; the Luiseño auxiliary occurs in sentential second position.

(4) #X AUX........

The major constituents of (2) and (3) may be reordered as in (5) and (6), respectively,[13] but none of the sentences is acceptable if the auxiliary is anywhere other than sentential second position, as (7) illustrates.[14]

(5) heyiqus̸ nil noo
I was digging.

heyin mo wunaalum
They will dig.

heyi xumpo wunaalum
They should dig.

(6) a. hunwuti nil noo patiqus̸
patiqus̸ nil noo hunwuti
I was shooting a bear.

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[13] I have written these sentences with the pronoun subject present. Actually, if a clause contains an auxiliary, the pronoun subject is present generally only if it is initial.

[14] The first person singular subject marker *n* usually cannot attach to anything but a first person singular pronoun subject.

b. hunwuti mo patin wunaalum
patin mo wunaalum hunwuti
They will shoot a bear

c. hunwuti xučapo čaam pati
pati xučapo čaam hunwuti
We should shoot a bear.

(7) a. *noo heyiqus̸ nil
*heyiqus̸ noo nil

b. *wunaalum hunwuti mil patiqus̸
*hunwuti wunaalum mil patiqus̸
*wunaalum hunwuti patiqus̸ mil
*hunwuti patiqus̸ wunaalum mil

Deciding where second position is in sentences like (2) and (3) or (5) and (6) is relatively easy. The sentences contain only single word constituents — and only subjects, objects, and verbs at that. Not all sentences are like this, of course; an examination of sentences which are not reveals how second position is defined in Luiseño.

0.3. One characteristic of (2), (3), (5), and (6) can be immediately discounted as a factor in deciding where second position is. It is not the case that to count as a first element in a clause an element must be a subject, an object, or a verb. Adverbial elements, for example, can also count as the first element.

(8) hamuʔ up wiiwiš kwaʔq
already AUX wiwish is:eating
She is already eating her wiwish.

waxaam pil wunaal teetilʔya
yesterday AUX she spoke
She spoke yesterday.

0.4. An examination of sentences with longer and more complex constituents than those in (2), (3), (5), (6), and (8), suggests that there are two different ways to define sentential second position in Luiseño, listed below as Principle 1 and Principle 2.

Principle 1:
Put the auxiliary after the first word.

Principle 2:
Put the auxiliary after the first constituent.

In the first part of this paper I will support the claim that both principles are involved in positioning the auxiliary. The statement of the two principles avoids two issues which the second part of this paper faces. First, where is the auxiliary before it is put after the first word or after the first constituent? Second, is the auxiliary moved to second position or is whatever precedes the auxiliary moved around the auxiliary?

1.1. In (2), (3), (5), (6), and (8) the first word in the clause is indistinguishable from the first constituent. In constructions where such is not the case — noun and modifier constructions, postpositional phrases, possessive phrases, and adverbial and object complement clauses, it is possible to position the auxiliary EITHER after the first word or after the first constituent.

Adjectives in Luiseño tend to follow the noun they modify.

(9) wiiwiš ˀaxaat
wiwish delicious
delicious wiwish

An auxiliary can come either after the noun or at the end of the entire constituent, after the adjective.

(10) [N AUX Adj]
[N Adj] AUX

(11) wiiwiš up ˀaxaat naˀq
wiwish AUX delicious is:burning

wiiwiš ˀaxaat up naˀq
wiwish delicious AUX is:burning
The delicious wiwish is burning.

Quantifiers generally precede the modified noun.

(12) muyuk kiiča
many house
many houses

An auxiliary can follow the quantifier or the entire noun phrase.

(13) [Quantifier AUX N]
[Quantifier N] AUX

(14) muyuk amil kiiča mulˀya
many AUX house burned

muyuk kiiča amil mulˀya
many house AUX burned
A lot of houses burned down.

Demonstratives, like quantifiers, generally precede the noun.

(15) ˀiviˀ ˀawaal
this dog
this dog

An auxiliary can either follow the demonstrative or the entire noun phrase.

(16) [Demonstrative AUX N]
[Demonstrative N] AUX

(17) ˀiviˀ up ˀawaal waˀiq
this AUX dog is:barking
ˀiviˀ ˀawaal up waˀiq
this dog AUX is:barking
This dog is barking.

A postposition can be attached directly to its object.

(18) wanii-yk
river-postposition
to the river

But if the object of the postposition is an animate noun, the postposition is attached to a pronoun copy of the noun object.

(19) nokaamay po-ˀeeš
my:son him-with
with my son

Should the postpositional phrase be initial to the sentence, an auxiliary can come after the noun object or after the postposition with its pronominal copy.

(20) [N AUX Postposition]
[N Postposition] AUX

(21) nokaamay nil poˀeeš monquś
my:son AUX him: with was:walking
nokaamay poˀeeš nil monquś
my:son him:with AUX was:walking
I was walking with my son.

Possessives in Luiseño are indicated by a possessive pronoun prefixed to the possessed noun.

(22) po-kaamay
his - son
his son

A noun possessor occurs with a third person possessive pronoun prefix.

(23) xwaan po-kaamay
John his-son
John's son

If the thing possessed is an animal, the possessive is prefixed to -*ˀaaš* instead of being directly prefixed to the possessed noun.

(24) no-ˀaaš ˀawaal
my-possessed:animal dog

xwaan po-ˀaaš ˀawaal
John his-possessed:animal dog
John's dog

In the possessive constructions of (23) and (24) the auxiliary may follow either the first word or the entire first constituent.[15]

(25) [N AUX........]
[N..........] AUX

(26) xwaan upil pokaamay wukoˀya
John AUX his:son came

xwaan pokaamay upil wukoˀya
John his:son AUX came
John's son came.

(27) noˀaaš up ˀawaal waˀiq
my:possessed:animal AUX dog is:barking

noˀaaš ˀawaal up waˀiq
my:possessed:animal dog AUX is:barking
My dog is barking.

(28) xwaan up poˀaaš ˀawaal waˀiq
John AUX his: possessed: animal dog is:barking

xwaan poˀaaš ˀawaal up waˀiq
John his:possessed:animal dog AUX is:barkint
John's dog is barking.

Before I consider the position of the auxiliary relative to a clause initial object complement clause or adverbial subordinate clause, let me discuss very briefly the construction of subordinate clauses in Luiseño. Usual verbal tense suffixes do not appear in subordinate clauses. Rather, the subordinate verb has affixed to it suffixes that indicate the time of the subordinate clause relative to the main clause. For example, in (29) the subordinate verb ends in *-nik*, a suffix which indicates that the time of the subordinate verb is prior to the time of the main verb, i.e., my seeing blood precedes my fainting.

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[15] There is one exception. The auxiliary cannot follow what looks like the first constituent in the possessive constructions: N possessive-name 'N's name'.

xwann up potuung kalak
John AUX his:name Calac
*xwaan potuung up kalak
John his:name AUX Calac
'John's name is Calac.'

It might be that these are really not possessive constructions like (23), but how they differ is certainly not obvious.

(29) noo nupo huluqan ˀowol tiiwi-nik
I AUX will:fall blood see-subordinator
I will faint when I see blood.

If the subject of the subordinate clause is different from the subject of the main clause, the subordinate verb generally has a possessive prefix as well.[16] For example, in (30) the subordinate verb has prefixed to it the first singular possessive marker — and the subordinate verb has a suffix *-pi* which indicates that the subordinate clause is unrealized at the time of the main clause, i.e., my sweeping follows John's telling me to sweep.

(30) xwaan ney yaa kiiš no-waki-pi
John me told house my-sweep-subordinator
John told me to sweep the house.

(29) illustrates an adverbial subordinate clause; (30), an object complement.

When object complement clauses and adverbial subordinate clauses are initial to a sentence, the auxiliary for the main clause generally follows the subordinate clause directly, but it can follow the first word of the subordinate clause.

(31) [[ ] AUX]
[[X AUX]]

(32) ˀowol tiiwinik nupo huluqan
blood see:subordinate AUX will:fall

ˀowol nupo tiiwinik huluqan
blood AUX see:subordinate will:fall
I will faint when I see blood.[17]

(33) kiiš nowakipi upil xwaan ney yaa
house my:sweep:subordinate AUX John me told

kiiš upil nowakipi xwaan ney yaa
house AUX my:sweep:subordinate John me told
John told me to sweep the house.

---

[16] With one subordinate verb suffix, the subordinate verb does not take a possessive even if it has a different subject than the verb of the main clause; this suffix occurs in the complements of verbs of perception.

čaam tiiwyax hunwuti huluqa-qal
we saw bear(object) fall-subordinator
'We saw the bear fall.'

[17] Note that the pronominal subject of the main clause has been deleted.

It is not immediately obvious from an examination of (32) that the auxiliary in the subordinate clause actually is the auxiliary of the main clause, but it is clear in (33). The subjects of the two clauses in (33) are different; the subordinate clause has a first person singular subject and the main clause has a third person singular subject. The auxiliary in (33) agrees with the subject of the main clause, not with the subject of the subordinate clause; *up* is the third singular clitic subject pronoun. The point is moot in any case, since subordinate clauses do not have an auxiliary.[18]

(34) *wunaal up yaˀaniq hunwuti up tiiwinik
he AUX is:running bear(obj) AUX see:subordinate

wunaal up yaˀaniq hunwuti tiiwinik
he AUX is:running bear(obj) see:subordinate
He is running away because he sees a bear.

It is not possible to argue that there is something peculiar to the first constituent in all these constructions (noun and modifier constructions, postpositional phrases, possessive phrases, and adverbial and object complement clauses) that allows the auxiliary to follow the first constituent rather than the first word. The construction types differ in form, in function and, depending on how one analyzes this sort of thing, in what they might be labeled in an underlying representation. Nor is it possible to argue that there is something peculiar to the first word in all these constructions that allows the auxiliary to follow it rather than the entire first constituent, at least as an option. First, the first words do not share a particular relationship to the rest of their respective constituents. In Noun Modifier constructions and postpositional phrases, the first word is the noun, the head of the construction; in Quantifier Noun or Demonstrative Noun constructions, the first word is the quantifier or demonstrative, probably not the head; in possessive constructions the first word is the most embedded constituent; in the subordinate clauses above, the first word is the object of the subordinate clause. Second, it is possible in the first three constructions under consideration to reverse the order of constituents without changing the positional possibilities for the auxiliary.

(35) a. ˀaxaat up wiiwiš naˀq
delicious AUX wiwish is:burning

ˀaxaat wiiwiš up naˀq
delicious wiwish AUX is:burning
The delicious wiwish is burning.

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[18] The first sentence in (34) is only an approximation of what might be the auxiliary in a subordinate clause were one to occur. Since the time of the subordinate clause is relative to the main clause, it is not obvious that the subordinate clause would take, in this case, a present tense auxiliary.

b. kiiča amil muyuk mulʔya
house AUX many burned

kiiča muyuk amil mulʔya
house many AUX burned
A lot of houses burned down.

c. poʔeeš nil nokaamay monquꞩ
him:with AUX my:son was:going

poʔeeš nokaamay nil monquꞩ
him:with my:son AUX was:going
I was walking with my son.

It is usually possible to reorder the elements of a subordinate clause, but these get less acceptable when the auxiliary is intercalated in the subordinate clause as well.

(36) noo nupo huluqan tiiwinik ʔowol
I AUX will:fall see:subordinate blood
I will faint when I see blood.

(37) tiiwinik nupo ʔowol huluqan
see:subordinate AUX blood will:fall
I will faint when I see blood.

So, it is clear that both counting words (Principle 1) and taking account of constituency breaks (Principle 2) are considerations in determining the position of the auxiliary in Luiseño and, thus, in determining sentential second position. Actually, a clarification of Principle 2 is necessary for it to be absolutely correct. It is certainly possible that the first constituent in a sentence will itself contain constituency breaks. For example, (38) probably has an intermediate constituency break as in (39).

(38) ʔawaal yawaywiš popuš
dog beautiful his:eyes
the beautiful dog's eyes

(39) [[ʔawaal yawaywiš] popuš]

The auxiliary cannot occur in the intermediate constituency break; it can only come after the first word or the ENTIRE constituent.

(40) ʔawaal up yawaywiš popuš ney toowwun
dog AUX beautiful his:eyes me are:looking

*ʔawaal yawaywiš up popuš ney toowwun
dog beautiful AUX his:eyes me are:looking

ʔawaal yawaywiš popuš up ney toowwun
dog beautiful his:eyes AUX me are:looking
The beautiful dog's eyes are looking at me.

1.2. There are exceptions to the generalization that EITHER Principle 1 or

Principle 2 can position the auxiliary. In these exceptional cases the auxiliary can occur only in the position defined by Principle 1 or only the position defined by Principle 2.

Principle 1 does not hold when the first word in the sentence is part of a coordinate structure. Nouns can be conjoined in Luiseño with the conjunction *piˀ* 'and'.

(41) xwaan piˀ mariya
John and Mary
John and Mary

The auxiliary cannot come after the first word in an NP like (41); it must follow the entire constituent.

(42) [N piˀ N] AUX

(43) xwaaan piˀ mariya pum ˀowoˀaan
John and Mary AUX are:working

*xwaan pum piˀ mariya ˀowoˀaan
John AUX and Mary are:working
John and Mary are working.

Principle 2 does not hold when the first constituent in a sentence is a sentential subject. The basic outlines of subordinate clause formation were discussed in 1.1. The formation of subject complements is like the formation of other subordinate clauses. In (44) the subordinate verb has the *-pi* suffix which indicates unrealized action [see (30)] and a possessive prefix, as expected since a sentential subject necessarily has a different subject from the main verb.

(44) wunaalum pom-ngee-pi miy-q
they their-leave-subordinate be-present:singular
They have to leave.

In sentences with subject complements the auxiliary for the main clause comes after the first word of the complement clause, not after the entire complement clause.

(45) [[X AUX]]

(46) wunaalum up pomngeepi miyq
they AUX their:leave:subordinate is

*wunaalum pomngeepi up miyq
they their:leave:subordinate AUX is
They have to leave.

In the good sentence in (46), following the third plural subject of the subject complement, there is an auxiliary composed of a third person singular subject marker, matching the third singular subject of the main verb — the sentential subject itself.

These exceptions are not without reason, but rather are instances of either Principle 1 or Principle 2 running afoul of a more general language tendency. This claim will be examined and supported as part of Section 2.

2.1. I have presented some data on the position of the auxiliary in Luiseño. It should be obvious that sentential second position is very much a surface phenomenon. (3) above illustrates the regular OSV word order; however, as other examples have shown, word order is relatively free. Whatever word order arrangements occur must precede the positioning of the auxiliary and, thus, the determination of second position. It is fair, then, to raise the two questions noted at the beginning of this paper. Where is the auxiliary before it gets put in second position? and, is the auxiliary moved to second position or is something moved around the auxiliary?

2.2. The auxiliary is initial to the clause, before it gets put in sentential second position.

(47) #AUX........

There are two sentence types in which the auxiliary actually is initial to its clause. I discussed above the position of the auxiliary in sentences with clause initial object complement clauses or adverbial subordinate clauses. In these, the auxiliary occurs initial to its clause, the clause it is the auxiliary for, when it immediately follows the subordinate clause.

(48) [[ ] AUX]

This position for the auxiliary is only one of the options, as noted, but the point stands nonetheless. The second type is conjoined sentences. *piˀ* 'and' can conjoin two sentences.

(49) temet kariˀaq piˀ ˀehengmayum xiivan
sun is: coming:up and birds are:singing
The sun is coming up and the birds are singing.

The auxiliary generally directly follows the conjunction.

(50) temet up kariˀaq piˀ pum ˀehengmayum xiivan
sun AUX is:coming:up and AUX birds are:singing

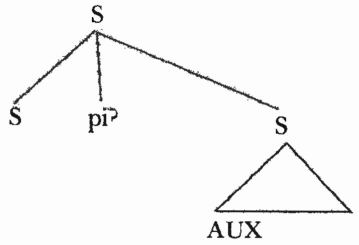
Whether we analyze conjoined sentences as in (51) or (52) below, the conjunction is outside the clause of which the auxiliary is a part.[19]

---

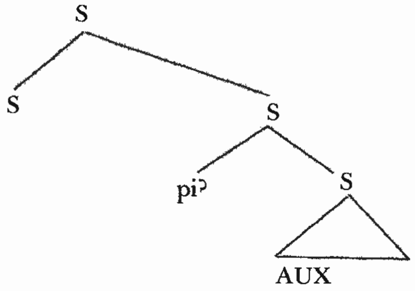
[19] There is good diachronic evidence for (52), but presenting it would take us far afield. That the conjunction does go with the second clause is suggested by its use as a sentence introducer.

piˀ ˀehengmayum xiivan
and birds are:singing
The birds are singing.

(51)



(52)



Therefore, the auxiliary in sentences like (50) is actually initial to its clause.

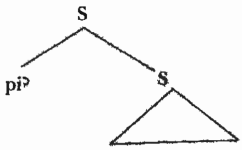
A somewhat different type of evidence has to do with a closely related language, Serrano. Serrano has an auxiliary much like the Luiseño auxiliary; unlike the Luiseño auxiliary, the Serrano auxiliary is not uncommonly in clause initial position.[20]

(53) maytavɨʔ ma:ç
AUX hear
He might have heard it. (Crook, 11)

As will be shown in 2.3 below, the development of a second position auxiliary from a clause initial auxiliary has an explanation; it is not obvious that a second position cliticized auxiliary could develop to a clause initial auxiliary.

Finally, when the auxiliary in Luiseño begins with *tee* or *man*, both of which can mean 'maybe', the auxiliary is initial to the clause.

Whether these sentences should also be analyzed as



is a question I will not attempt to answer here.

[20] The Walbiri auxiliary may also be initial under certain conditions (see Hale 1973).

(54) tee=nu=po noo ngeen
maybe=SM=future I will:leave
Maybe I will leave.

(55) man=xu=po wunaal ngeen
maybe=modal=future he will:leave
Maybe he should leave.

2.3. The second question raised at the beginning of this section had to do with whether the auxiliary is moved to second position or the sentence initial element is moved around the auxiliary. There are cases that might very well be argued to involve movement of some element around the auxiliary. On the other hand, it is by no means clear that movement around the auxiliary can be argued for in the majority of cases.

One case that argues for movement around the auxiliary is the position of the auxiliary relative to clause initial coordinate structures. As noted, the auxiliary cannot occur after the first word of a coordinate sturcture. Ross (1967) has argued that there is a general constraint on movement rules prohibiting movement out of a coordinate structure. The ungrammaticality of English sentences like (56) follows from the constraint.

(56) *Who did John kick Bill and?

The coordinate structure constraint is given below, labeled Principle 3.

Principle 3:
In a coordinate structure, no conjunct may be moved, nor may any element contained in a conjunct be moved out of that conjunct.

That coordinate structures in Luiseño should be exceptions to Principle 1 follows directly from Principle 3, *if* we assume that there is movement of the first word around the auxiliary. Principle 3 is hypothesized to be a language-general constraint; thus, the Luiseño Principle 1 runs afoul of a more general principle and the more general principle is dominant.

This argument is not totally trouble-free, however. Just as no conjunct may be moved out of a coordinate structure, it is certainly possible that nothing — like an auxiliary — may be moved into a coordinate structure. That is, the explanation for why the auxiliary cannot occur after the first word of a coordinate structure — and Principle 3 itself — follows from an even more general principle.

Principle 4:
Coordinate structures resist disruption.

Although the case for movement around the auxiliary is thus weakened, the position of the auxiliary relative to coordinate structures is not without reason. The more general principle — whether it be Principle 3 or Principle 4 — wins out over the more language-specific principle, Principle 1.

If Principle 3 or Principle 4 is dominant over Principle 1, another construction in Luiseño demands explanation. Structures like those in (57) are glossed with 'and'.

(57) xwaan mariya weh
John Mary weh
John and Mary

The auxiliary may come after the first word or after *weh*.

(58) xwaan pum mariya weh ˀowoˀaan
John AUX Mary weh are:working

xwaan mariya weh pum ˀowoˀaan
John Mary weh AUX are:working
John and Mary are working.

If this actually is a coordinate structure, we would have to say that Principle 3 (or Principle 4) is dominant only in some coordinate structures. But there are other curious characteristics of constructions like (57). First, when this sort of construction is the object in a sentence, *weh* takes object marking, but the two nouns do not.

(59) noo n toowq xwaan xosee wehwunmi
I AUX see John Jose weh:object

*noo n toowq xwaan-i xosee-i $\left\{\begin{array}{l}\text{wehwunmi}\\ \text{weh}\end{array}\right.$

I AUX see John-object Jose-object weh
I see John and Jose.

Nouns conjoined by *piˀ* do take object marking.

(60) *noo n toowq xwaan piˀ xosee
I AUX see John and Jose

noo n toowq xwaan-i piˀ xosee-i
I AUX see John-object and Jose-object
I see John and Jose.

Second, *weh* is the Luiseño word for 'two'. It is possible, then, that in constructions like (57) *weh* is acting as a resumptive element for the nouns which precede it; *weh*, then, may actually be the head of the construction. If *weh* is the head, then perhaps the nouns in constructions like (57) are not really conjoined but simply listed. And, if we look at actual lists, there are two positional possibilities for the auxiliary. Principle 1 can put the auxiliary after the first word or Principle 2 can put the auxiliary after the first constituent.

(61) xwaan pum mariya felisita čoˀonum monaa čeyk
John AUX Mary Felicita all are:going here

xwaan mariya felisita pum čoˀonum monaa čeyk
John Mary Felicita AUX all are: going here
John, Mary, Felicita, all are going.

The second argument for movement around the auxiliary has to do with the possibility that there is some emphasis or focus or topic notion (using the terms very loosely) associated with what precedes the auxiliary. In general, for the cases discussed in 1.1, there appears to be no difference in meaning between those sentences with the auxiliary positioned by Principle 2, or, perhaps, it is a difference so subtle that I cannot get at it. However, in some very few cases, and only sporadically, there is something like one of the three notions above associated with the element that precedes the auxiliary. (I will use the term *emphasis* as a cover term.) For example, the two sentences below differ only in the position of the auxiliary. The sentences were given different glosses.[21]

(62) xwaan pomiix waniiča up poloov
John his:thing river AUX good
John's river is nice.

(63) xwaan up pomiix waniiča poloov
John AUX his:thing river good
That is *John's* river and it is nice.

And there is synchronically a sort of emphasis potentially associated with sentence initial position. If two things are contrasted as in sentences like *I know John but I don't know Mary*, the contrasted elements tend to be initial.

(64) xwaani noo ˀayaliq piˀ mariyi qay noo ˀayaliq
John:object I know and Mary:object negative I know
I know John but I don't know Mary.

Finally, in questions the question marker is in sentential second position and the questioned element tends to precede it.[22]

(65) ˀawaali ꞩu xwaan ˀariq
dog:object Q xwaan is:kicking
Is John kicking the dog?

---

[21] (62) and (63) contain another possessive construction. With things that usually are not possessed, like rivers, the possessive prefix is attached to *-miix*.

[22] The question marker argument is probably weak. Question markers show many of the same positional possibilities as the auxiliary, that is, they can follow quantifiers and such. Therefore, the questions that arise for the auxiliary arise for the question marker too.

Thus, there is reason to expect that an emphasized element is moved to the front of the sentence, around the sentence initial auxiliary.

If it is true that there is something like emphasis associated with preceding the auxiliary, the exceptional nature of sentential subject complements relative to Principle 2 has some explanation. Principle 5 is a statement of what I would like to suggest is a language-general tendency.

> Principle 5:
> Avoid sentential subjects, at least at the beginning of a sentence.

It is possible to view extraposition in English as an instance of this principle. Munro (1974) has commented on a general tendency in Yuman languages to avoid sentential subjects. Specific to Luiseño, sentential subjects are not particularly stable. In (66) the object of the sentential subject has drifted out of the complement clause to the right of the embedding verb.

(66) čaam čams̸eˀipi miyq nooti
we our:choose:subordinate is chief:object
We have to choose a chief.

If it is true that emphasis or some like notion is associated with the element that precedes the auxiliary, for an entire sentential subject to precede the auxiliary would be to emphasize the sentential subject — and concomitantly the fact that there is a sentential subject. If sentential subjects are, in general, to be avoided, at least at the beginning of a sentence, it is not likely that something which has the effect of emphasizing them would be permitted. Thus, the Luiseño Principle 2 runs afoul of the more general Principle 5 and Principle 5 is dominant.[23]

These arguments for movement around the auxiliary have some appeal. It is also true that, regardless of whether the auxiliary moves or something moves around the auxiliary, the grammar of Luiseño is going to have to be able to identify both words and constituency breaks of a particular sort. But there are a number of problems with the suggestion that everything which precedes the auxiliary is moved there. First, in most cases there is no emphasis associated with what precedes the auxiliary. If there is no clear synchronic emphasis, it is not clear to me that generalized movement around the auxiliary can be justified.[24] Second, in those instances where only the first word of a complex constituent precedes the auxiliary, it would be necessary to first move the entire constituent to the position immediately following the auxiliary, since sentences like the first one below are no good.

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[23] I have included as proof of Principle 5 two processes with very different effects. Extraposition allows a sentential subject to get away from the beginning of the sentence. Drifting part of the sentential subject past the embedding verb destroys the clause boundaries of the sentential subject. In light of this, whether I am justified in considering them both instances of a single more basic language principle is an arguable point. Hopefully, it is also an empirical question.

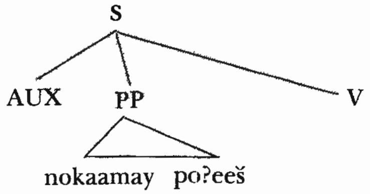
[24] The auxiliary is unstressed. It might be possible to argue that an unstressed element

(67) *pokaamay upil monqus̸ poˀeeš
his son AUX was:going him:with

pokaamay upil poˀeeš monqus̸
his:son AUX him:with was:going
He was going with his son.

I can think of no justification for such a proposal. Finally, even if I knew what it meant to emphasize a demonstrative or a quantifier, I do not know what it would mean to argue in postpositional phrases or in subordinate clauses, for instance, that a particular word has been moved out of the phrase or clause. Assume that for sentences like (21) and (32) we have structures something like (68) and (69), respectively.

(68)



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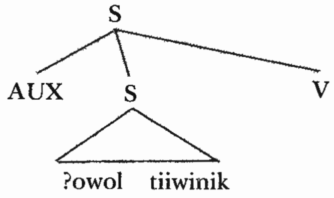

Is the first word in each of these cases moved around the auxiliary to attach to the S? (70) and (71) seem rather strange. We could get around the problem by arguing that, since positioning the auxiliary is very much a surface phenomenon, it is not structures like (68) and (69) that movement occurs in; rather, the sentence is at that point a purely linear phenomenon, as exemplified in (72) and (73).

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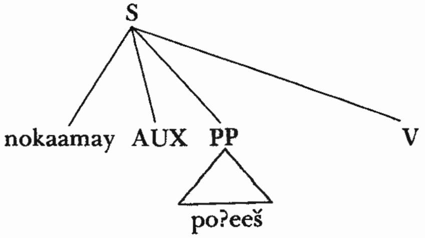
cannot begin a sentence; therefore, something has to be moved around the auxiliary. But, and equally possible, it could be argued that the auxiliary has to move around something because it is unstressed.

(70)



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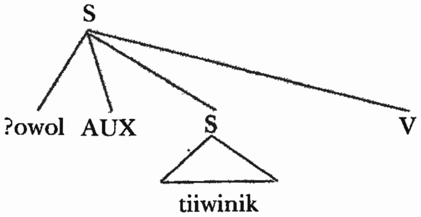

(72) AUX nokaamay poʔeeš........

(73) AUX ʔowol tiiwinik........

There can be no denying that a sentence becomes linear at some point in its production; the speaker has to begin at the beginning and progress to the end. A number of explanations of various language phenomena have depended in part on the notion that speech is linear. (See Lakoff 1969; and Langacker 1969.) The question is WHEN a sentence becomes linear. I cannot answer the question; therefore, it is impossible here and now to prove — or to disprove — the hypothesis as it pertains to the positioning of the auxiliary.

The best explanation for the fact that movement around the auxiliary is justifiable in some cases and not in others is that the synchronic situation represents various stages of a diachronic process. Assume that at some earlier stage of Luiseño the position in front of the auxiliary was the position of an emphasized element; an emphasized element was moved to clause initial position as indication of its emphasis.[25] The possibility of emphasizing something would be strongly militated against at least in exactly those sentences where the auxiliary is synchronically initial to its clause. The conjunction *piʔ* regularly introduces sentences in connected texts with a single topic. The subordinating suffix on the verb of subordinate clauses indicates time and subject of the subordinate clause relative to the main clause. Emphasizing some part of the main clause when it followed such indication should at least be difficult. Although modal particles in Luiseño regularly derive from conjunctions, *man* and *tee* are closer to conjunctions synchronically than are other modal elements. Thus, the clause initial auxiliary in sentences like (54) and (55) may be due to the same tendency away from emphasizing an element in a sentence which begins with a conjunction. Assume now that the emphasis notion connected with immediately preceding the auxiliary began to disappear. At this point the auxiliary began to be moved around the first element in the clause, perhaps not in all constructions at the same time. It is likely that constituents, although perhaps primarily single word constituents, would have been the basic unit to be emphasized. However, once the emphatic notion was weakened — or lost — sentential second position could generalize from the first constituent to the first word, regardless of constituency break.

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[25] The auxiliary itself may have been initial, but probably only when it included modality. The explanation for such a synchronic situation is long and complex, involving consideration of the creation of the auxiliary; it is far beyond the scope of this paper.

3. I have discussed how second position is defined in Luiseño. The definition has involved three basic points. First, given Principles 1 and 2, the grammar of Luiseño has to be able both to count and to recognize constituency breaks of a particular sort. Second, given the interaction of Principles 1 and 2 with Principles 3, 4, and 5, the definition of second position is in part subject to principles which are language general. Third, the synchronic definition of second position reflects its diachronic development.

I stated at the beginning of this paper that sentential second position is important cross-linguistically. I do not expect that second position will be defined universally as it is in Luiseño. Some languages will undoubtedly refer only to the first word; others only to the first constituent.[26] Perhaps still others will combine both possibilities, but will use different criteria to combine them. We need to know how languages decide where second position is and what, if anything, that decision correlates with.

At the beginning of this paper I noted that hypotheses about the reasons for the importance of sentential second position often appealed to the importance of sentence initial position. By arguing that the auxiliary in Luiseño is generated initially, this paper implicitly supports such appeals. It is important, however, not to obscure the importance of sentential second position itself with either the argument for clause initial generation of the auxiliary or the arguments for how the auxiliary gets into sentential second, either synchronically or diachronically. By occurring after an emphasized element, the Luiseño auxiliary, at least diachronically, partitions the sentence into two sections, one approximating topic and the other approximating comment. With the auxiliary enclitic to the first element in the clause, a mass of important information appears in sentential second position — a judgment as to the potential reality of the situation described in the sentence, the person and number of the subject, the tense of the sentence.

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[26] Other possibilities exist as well, albeit much less frequently. In Pashto, the second position clitics can follow the first stressed syllable of a clause initial verb, a syllable which is not a prefix.

Linguistic theory is ultimately going to have to recognize the cross-linguistic importance of sentential second position. By examining sentential second position in some detail in one language, I hope to have hurried the day.

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Categorial Grammar and the Order of Meaningful Elements*

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From the work of Greenberg (1966), Lehmann (1972a, b, c, 1973), Bartsch and Vennemann (1972), and Vennemann (1972, 1973a, b, 1974a, b, 1975)[1] a certain concept of consistent basic serialization (CBS)[2] has developed which is understood as follows: a language has CBS if specifiers (modifiers, qualifiers, *déterminants, unterscheidende Glieder*, operators) either all precede or all follow the specified (modified, qualified, *déterminé, unterschiedene Glied,* operand).[3] A language with CBS is called a consistent prespecifying language (*langue montante* or *centripète,* OV language, XV language) if specifiers precede the specified; otherwise it is called a consistent postspecifying language (*langue descendante* or *centrifuge*, VO language, VX language).[4] Examples of specification are the relation of various kinds of complements and attributes to their head expressions, e.g. that of object and verb, adverbial and verb, adnominal and noun, object and adposition.[5]

The goal of this paper is to give a precise definition of the concept of specification and, consequently, of CBS in a simple categorial grammar,[6]

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* I am grateful to the participants of my seminar on serialization, University of Munich, Winter 1975-76, especially to Matthias Varga von Kibéd, for numerous helpful criticisms of a draft version of this paper.

[1] Many older works are relevant to this development. Some of them are discussed in detail in Vennemann MS.

[2] The term *consistent* is Lehmann's. Tesnière uses *accusé*.

[3] The terms are used e.g. in Vennemann (1973b), Fries (1940), C.L. Lewis (1967), Trubetzkoy (1939), Behaghel (1932), Bartsch and Vennemann (1972), respectively.

[4] The terms in parentheses are employed in Tesnière (1969), Greenberg (1966), and Vennemann (1973b), respectively.

[5] Examples are given in the works cited in the first sentence above. A list of some categories and subcategories occurring in the relation of specification is given in Vennemann (1972, 1974a).

[6] The categorial grammar is of the kind used in Ajdukiewicz (1935-36), Lambek (1958), Bar-Hillel (1959), D. Lewis (1972), Montague (1973), Bartsch and Vennemann (1973),

relative to the assumption (which is not quite true to fact) that the concept of basic serialization (BS), i.e. 'normal' or 'unmarked' element order, has been made precise beforehand.

The categorial grammar I use is taken from D. Lewis (1972), except that linear order plays no role in it. Its basic categories are *n, v,* and *c*, where the symbols are chosen mnemonically to remind the reader of *noun phrase*, *verb*, and *common noun*, respectively.[7] Where D. Lewis uses *s* I use *v* to prevent confusion with the idea that this category stands for that of *(declarative) sentences*. D. Lewis (1972:205f.) distinguishes between the category *s* of *sentence radicals* and the category *sentence*, the latter comprising a *sentence radical* and a *mood* (*declarative, imperative, interrogative*). E.g., the sentences

> You are industrious.
> Be industrious!
> Are you industrious?

differ only in *mood* while having the same *sentence radical*. Alternatively, one could have three categories of sentences, *dec*, *imp*, and *int*, and define mood as *dec/s*, *imp/s*, and *int/s*, respectively.

D. Lewis does not justify the differentiation between *s* and the sentences beyond the idea that the sentence types exemplified above have something in common that "specifies a state of affairs" while differing only in "whether the speaker is declaring that the state of affairs holds, commanding that it hold, asking whether it holds, or what". There exists, however, additional justification for such a distinction. In the past one has tried to distinguish between adsententials and adverbials by assigning them to the categories *s/s* and *(s/n)/(s/n)*, respectively. This may work for examples such as the following; cf. figures 1 and 2.

> Unfortunately Mary is screaming violently.
> *Violently Mary is screaming unfortunately.

However, such a distinction is impossible in the case of sentences with basic zero-place verbs, such as the following, where the Latin example

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Bartsch (MS), Cresswell (1973). It differs from that in Lambek, Bar-Hillel, D. Lewis, and Cresswell in that linear order is not represented in it (e.g., both $s/n \cdot n$ and $n \cdot s/n$ reduce to s). It differs from Montague, Bartsch and Vennemann, Bartsch, and Cresswell in that *s* is not identified with the category of (declarative) sentences.

[7] For the purpose of this paper it is irrelevant whether categories are understood as sets of expressions or as indices labeling sets of expressions. I treat them as indices, following Montague (1973). — It will be seen later that it is irrelevant for the purpose of this paper that three categories are assumed as basic. One could try to make do with two, or have more than three. The definition of specification given below is neutral as to this question.

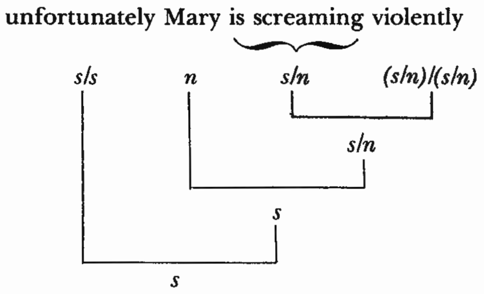

Figure 1.

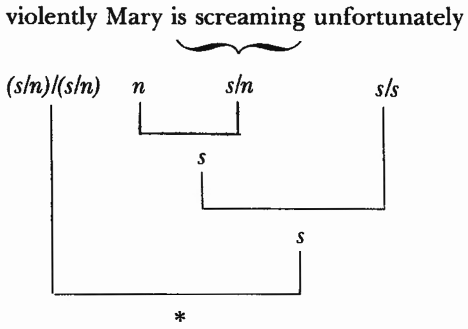

Figure 2.

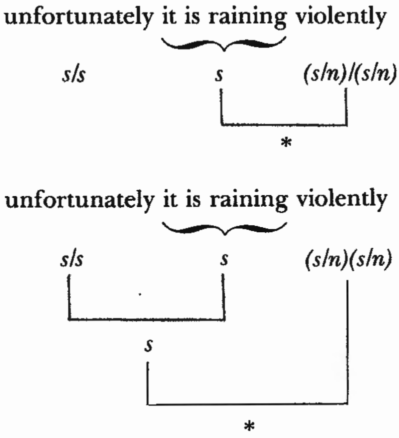
unfortunately it is raining violently
s/s
s
(s/n)/(s/n)
*
unfortunately it is raining violently
s/s
s
(s/n)(s/n)
s
*

Figure 3.

shows the zero-place character of the verb more clearly than the English one with its expletive *it*; cf. figure 3.

Unfortunately it is raining violently.
*Violently it is raining unfortunately.
Infeliciter violenter pluit.

The fundamental categorial difference between *violently* and *unfortunately* becomes evident in the following sets of examples:

You are screaming violently.
Be screaming violently! (Scream violently!)
Are you screaming violently?

It is raining violently.
(Let it be raining violently.)
Is it raining violently?

Unfortunately it is raining.
(*Let it unfortunately be raining.)
*Is it unfortunately raining?

What these sentences show is that *violently* serves to characterize the state of affairs and is independent of the mood of the sentence, while *unfortunately* expresses the speaker's attitude towards *the state of affairs declared to exist* (i.e., to an assumed fact). This difference can easily be expressed in the present system by assigning *violently* to the category *s/s* but *unfortunately* to the category *dec/dec*; cf. figures 4-7, where DEC, IMP, and INT are abbreviations for the devices that mark mood, e.g. a special intonation, an order inversion, or a morpheme (such as *ne* in a Latin *yes-no* question, e.g. *Pluitne?* 'Is it raining?'). This approach, incidentally, leaves open the question whether in a sentence such as *Mary is screaming violently* the adverbial *violently* specifies the zero-place verb *Mary is screaming*, as in figure 6, or the one-place verb *is screaming*, as in figure 8. Similarly, the approach permits three different analyses of the sentence *Mary is kissing John violently*, provided that the grammar assigns *violently* at least to the categories *s/s*, *(s/n)/(s/n)*, and *((s/n)/n)/((s/n)/n)*. I will not discuss here the problem whether, or to what extent, such multiple category assignment is needed on empirical grounds. Nor will I have much to say about the categories *dec*, *imp*, and *int* (or their respective *moods*).

It is evident from the above that the category *s* of *sentence radicals* is nothing but the category of zero-place verbs, either basic as in the case of *(it) rains*, *pluit*, or derived as in the case of *Mary screams, Mary kisses John, Mary gives John the book*. I.e., *gives* is assumed to be a three-place verb, *((s/n)/n)/n*, abbreviated v^3, because it requires three noun phrases to form a sentence radical; *gives the book* is assumed to be a two-place verb, *(s/n)/n*,

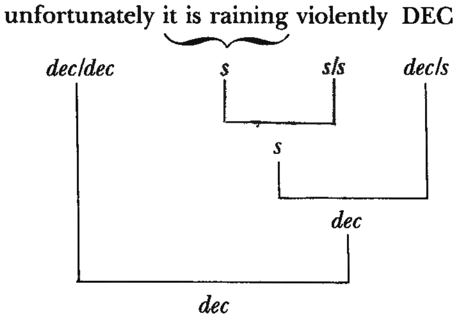

Figure 4.

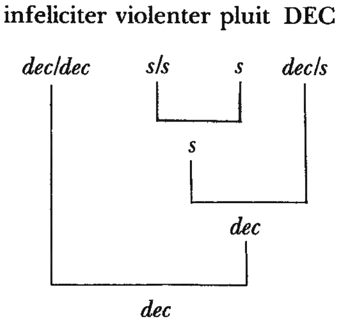

Figure 5.

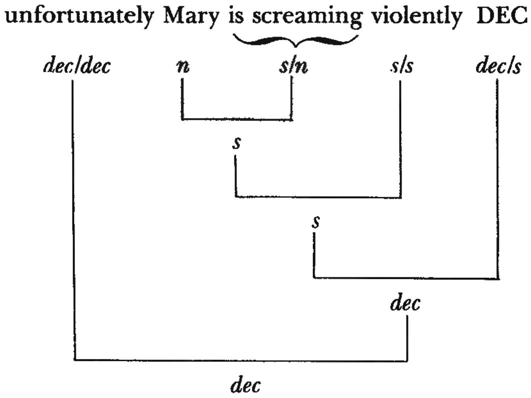

Figure 6.

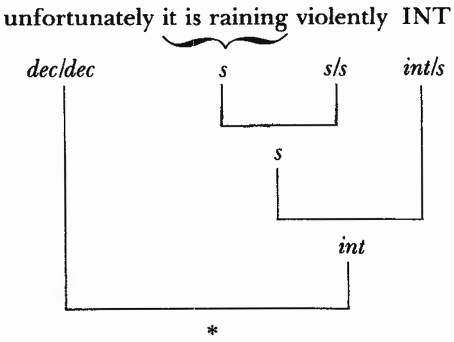

Figure 7.

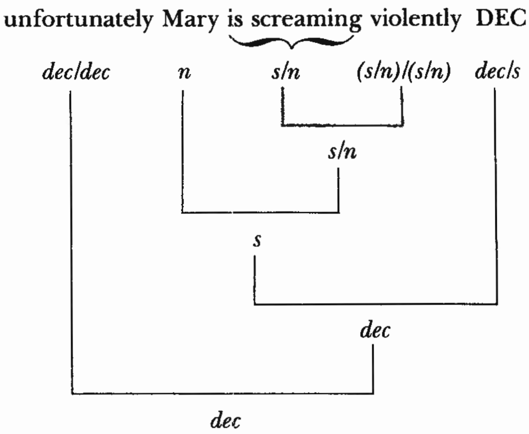

Figure 8.

abbreviated v^2, because it requires two noun phrases to form a sentence radical, just like *kisses*; *gives John the book* is assumed to be a one-place verb, s/n, abbreviated v^1, because it requires one noun phrase to form a sentence radical, just like *kisses John* and *screams*; *Mary gives John the book* is assumed to be a zero-place verb, s, or v^0, because it requires no noun phrase at all to form a sentence radical, just as *Mary kisses John, Mary screams*, and *(it) rains* or *pluit* do not require, indeed, do not admit any further noun phrases. Once the category of sentence radicals has been identified with that of zero-place verbs, v^0, for which simply v, there is no need to keep the symbol s, and I replace it by v.

I will now define recursively what I mean by a *sub-sentential category*, or simply a *category*.[8] I do this by defining the minimal set of symbols with certain properties.

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[8] I.e., I exclude sentential categories such as *dec, imp,* and *int*, because I would like to keep the difficult problem of the position of adsententials out of the definition of CBS.

DEFINITION of the set *Cat* of categories:

Cat is the intersection of all sets M such that

I. $n, v, c \in M$

II. If $h, k_1, k_2, \ldots, k_m \in M$ for some natural number m ($m \neq 0$), then $(h)/(k_1, k_2, \ldots, k_m) \in M$.

I.e., n, v, and c are categories; and if $h, k_1, k_2, \ldots, k_m$ are categories, then $(h)/(k_1, k_2, \ldots, k_m)$, i.e. the symbol composed of h enclosed by parentheses, followed by a slash, followed by the sequence $k_1, k_2, \ldots, k_m$ enclosed by parentheses, is a category. And there are no further categories. The categories *n, v,* and *c* are called *basic categories*; all other categories are called *derived categories*. The following are some examples of derived categories:

(1) $((v)/(n))/(n)$
(2) $(n)/(c)$
(3) $(n)/(v)$
(4) $(v)/(v,v)$
(5) $((v)/(n))/((v)/(n), (v)/(n))$

I will omit parentheses where no confusion can arise; e.g., I will write (1) $(v/n)/n$, (2) n/c, (3) n/v, (4) $v/(v,v)$, (5) $(v/n)/(v/n, v/n)$.

Let L be some natural language. Then L will have only a finite number of *basic expressions*. In a categorial grammar of L, each expression will have to be assigned one or several categories. The set of basic expressions to which a category h is assigned in this way is named B_h. Most categories will, of course, not be assigned to any basic expressions at all; in that case B_h will simply be empty. There will be syntactic rules in the grammar that take one or several expressions of certain categories and form new expressions, assigning them a category. The total set of expressions assigned a category h, whether basic or derived by a syntactic rule, is named P_h (the set of *proper expressions* of category h).[9] It follows that for any category h, B_h is contained in P_h. But there are categories h for which the converse is not true, and there may be categories h for which B_h is empty but P_h is not.[10]

E.g., if L is English, we will assign *kisses* (or *kiss-*, but I will ignore agreement for the present purpose) the category of two-place verbs, $(v/n)/n$, cf. (1); i.e. *kisses* $\in B_{(v/n)/n}$. The article *the* will be assigned the category n/c, *the* $\in B_{n/c}$, cf. (2), because *the* together with a common noun, e.g. *man* (*man* $\in B_c$), forms a noun phrase: *the man* $\in P_n$. The conjunction

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[9] This usage follows Montague (1973).

[10] E.g., there are languages without basic zero-place verbs, and others without basic adnominals. Had we included sentential categories, B_{dec} would be empty for English, while, of course, P_{dec} would not be.

that would be assigned the category n/v, cf. (3), because it builds noun phrases from zero-place verbs; e.g., *Mary screams* ϵP_v, *that* $\epsilon B_{n/v}$, *that Mary screams* ϵP_n, because the latter expression binds argument places of other expressions:

$$\left.\begin{array}{l}\text{it}\\ \text{the scream}\\ \text{this constant screaming}\\ \text{this fact}\\ \text{that Mary screams}\end{array}\right\}\ \text{bothers John}$$

The conjunction *or* would be assigned the categories $v/(v,v)$ and $(v/n)/(v/n, v/n)$, cf. (4), (5), because it forms (among other expressions) zero- and one-place verbs from pairs of zero-place verbs and from pairs of one-place verbs: *Mary screams* ϵP_v, *John cries* ϵP_v, *or* $\epsilon B_{v/(v,v)}$, *Mary screams or John cries* ϵP_v; *kisses John* $\epsilon P_{v/n}$, *screams* $\epsilon B_{v/n}$, *or* $\epsilon B_{(v/n)/(v/n,\, v/n)}$, *kisses John or screams* $\epsilon P_{v/n}$, because this expression, just like its constituents *kisses John* and *screams*, requires a noun phrase, e.g. *Mary*, to form a zero-place verb.

DEFINITION of multiplication for categories:

Let m be a natural number and $m \neq 0$. Let $k_0, k_1, \ldots, k_m$ be categories. I distinguish two cases:

(1) There is an r' $(0 \leqslant r' \leqslant m)$ and there is a category h such that $k_{r'}$ is $h/(k'_1, k'_2, \ldots, k'_m)$, where $(k'_1, k'_2, \ldots, k'_m)$ is the sequence that is derived from $(k_0, k_1, \ldots, k_m)$ by eliminating $k_{r'}$. (It is easy to see that if there is such an h, there is exactly one). Then

$$\prod_{r=0}^{m} k_r =_{\text{Def.}} h.$$

(2) Otherwise:

$$\prod_{r=0}^{m} k_r =_{\text{Def.}} 0.$$

The notation $k_0 \cdot k_1 \cdot \ldots \cdot k_m$ is also used for $\prod_{r=0}^{m} k_r$ and is preferred in the case $m = 1$.

$\prod_{r=0}^{m} k_r$ is called the categorial product of $k_0, k_1, \ldots, k_m$.

The following examples illustrate categorial multiplication:

[a] $n \cdot v/n = v$

[b] $(v/n)/n \cdot n = v/n$

[c] $v/n \cdot (v/n)/(v/n,\, v/n) \cdot v/n = v/n$

[d] $n/v \cdot v = n$

[e] $v/n \cdot (v/n)/(v/n) = v/n$

[f] $v/n \cdot (v/n)/(v/n, v/n) \cdot n = 0$

Proofs: the definitions of *Cat* and of multiplication.

RULE for the categorization of complex expressions:

Let m be a natural number and $m \neq 0$. Let $k_0, k_1, \ldots, k_m$ be categories. Let $\alpha^r \in P_{k_r}$ for $0 \leq r \leq m$. For convenience, I index expressions with the category assigned them; e.g., I write $\alpha^r_{k_r}$ for α^r with $\alpha^r \in P_{k_r}$. The expression derived from the sequence $(\alpha^0_{k_0}, \alpha^1_{k_1}, \ldots, \alpha^m_{k_m})$ by the rules of the grammar, e.g. serialization rules and sandhi rules, will be assigned $\prod_{r=0}^{m} k_r$. For convenience, the same may be done with the sequence itself and with all intermediate forms in the derivation; e.g., $(\alpha^0_{k_0}, \alpha^1_{k_1}, \ldots, \alpha^m_{k_m})_{\prod_{r=0}^{m} k_r}$. If $\prod_{r=0}^{m} k_r = 0$, we write $*X$ for X_0.[11]

DEFINITION of the predicate *CWB* ("categorially wellbehaved") for sequences of expressions:

Let $(\alpha^0_{k_0}, \alpha^1_{k_1}, \ldots, \alpha^m_{k_m})$ be as in the preceding rule.

$$CWB(\alpha^0, \alpha^1, \ldots, \alpha^m) \leftrightarrow_{\text{Def.}} \prod_{r=0}^{m} k_r \neq 0$$

$CWB(\alpha^0, \alpha^1, \ldots, \alpha^m)$ is read, "The sequence $(\alpha^0, \alpha^1, \ldots, \alpha^m)$ is categorially wellbehaved".[12]

It follows from the definition of categorial multiplication that if a sequence of expressions is categorially wellbehaved, so is every permutation of the sequence. — An expression γ that is derived from a categorially wellbehaved sequence of expressions by the rules of the grammar is called *categorially wellformed*, written CW(γ).

EXAMPLES from English:

[a′] $CWB(Mary_n, screams_{v/n})$

[b′] $CWB(kisses_{(v/n)/n}, John_n)$

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[11] Note that the way categorial multiplication has been defined, no index is assigned to incomplete expressions such as *walks along* where *along* is a preposition, i.e. of the category $((v/n)/(v/n))/n$. Assignment of an index could be desirable in a grammar treating expressions such as *the river that Mary walks along* and *Mary walks along, but John jumps across, the river* without transformations. In this case multiplication would have to be defined differently so as to yield, in this case, $v/n \cdot ((v/n)/(v/n))/n = (v/n)/n$. Since this problem is irrelevant to the question of the relationship between specification and basic serialization, I will not pursue it further. (This relation between categorial multiplication and possible analyses was pointed out to me by M. Varga von Kibéd).

[12] I omit one pair of parentheses after *CWB* to increase legibility. It must be borne in mind, however, that *CWB* is a one-place predicate taking sequences of expressions as arguments, rather than a multi-place predicate (with a variable number of arguments!). The same convention is used for the functions *head* and BS_L defined below.

[c′] $CWB(screams_{v/n},\ or_{(v/n)/(v/n,\ v/n)},\ kisses\ John_{v/n})$
[d′] $CWB(that_{n/v},\ Mary\ screams_{v})$
[e′] $CWB(screams_{v/n},\ violently_{(v/n)/(v/n)})$
[f′] not $CWB(screams_{v/n},\ or_{(v/n)/(v/n,\ v/n)},\ Johnn)$

Proofs: Def. "*CWB*", with [a] - [f] above.

DEFINITION of the head of a categorially wellbehaved sequence of expressions:

Let $(\alpha^0_{k_0}, \alpha^1_{k_1}, \ldots, \alpha^m_{k_m})$ be as in the preceding rule and definition. Let $CWB\ (\alpha^0, \alpha^1, \ldots, \alpha^m)$. It follows from the preceding that

$$\prod_{r=0}^{m} k_r \neq 0.$$

It follows from the definition of multiplication that there is exactly one r $(0 \leqslant r \leqslant m)$ such that there is a category h such that

$$k_r = h/(k'_1, k'_2, \ldots, k'_m)$$

where $(k'_1, k'_2, \ldots, k'_m)$ is derived from $(k_0, k_1, \ldots, k_m)$ by eliminating k_r. Let this r be r'. I distinguish two cases:

(1) $(h) \neq (k'_1, k'_2, \ldots, k'_m)$

Then: $head(\alpha^0, \alpha^1, \ldots, \alpha^m) =_{\text{Def.}} \alpha^{r'}$

(2) $(h) = (k'_1, k'_2, \ldots, k'_m)$

In this case, $m = 1$; r' is either 0 or 1. Let r'' be the other of the two, i.e. $r'' = 1 - r'$. Then:

$head(\alpha^0, \alpha^1, \ldots, \alpha^m) =_{\text{Def.}} \alpha^{r''}$

$head(\alpha^0, \alpha^1, \ldots, \alpha^m)$ is read "the head of the sequence $(\alpha^0, \alpha^1, \ldots, \alpha^m)$".

EXAMPLES from English:

[a″] $head(Mary_{n},\ screams_{v/n}) = screams_{v/n}$
[b″] $head(kisses_{(v/n)/n},\ John_{n}) = kisses_{(v/n)/n}$
[c″] $head(screams_{v/n},\ or_{(v/n)/(v/n,\ v/n)},\ kisses\ John_{v/n}) = or_{(v/n)/(v/n,\ v/n)}$
[d″] $head(that_{n/v},\ Mary\ screams_{v}) = that_{n/v}$
[e″] $head(screams_{v/n},\ violently_{(v/n)/(v/n)}) = screams_{v/n}$

Proofs: Def. "*head*", with [a′] - [e′].

DEFINITION of complementation and attribution:

Let $(\alpha^0_{k_0}, \alpha^1_{k_1}, \ldots, \alpha^m_{k_m}), h, k'_1, \ldots, k'_m$ be as in the definition of the head; in particular, let $CWB(\alpha^0, \alpha^1, \ldots, \alpha^m)$. Let t $(0 \leqslant t \leqslant m)$ be such that $\alpha^t = head(\alpha^0, \alpha^1, \ldots, \alpha^m)$. Let r be such that $0 \leqslant r \leqslant m$ and $r \neq t$.

$$COMP(\alpha^r, \alpha^t) \longleftrightarrow_{\text{Def.}} (h) \neq (k'_1, k'_2, \ldots, k'_m)$$
$$ATTR(\alpha^r, \alpha^t) \longleftrightarrow_{\text{Def.}} (h) = (k'_1, k'_2, \ldots, k'_m)$$

$COMP(\alpha^r, \alpha^t)$ [$ATTR(\alpha^r, \alpha^t)$] is read, "α^r is a complement [an attribute] of (the head) α^t".

EXAMPLES from English:

[a'''] $COMP(Mary_n, screams_{v/n})$
[b'''] $COMP(John_n, kisses_{(v/n)/n})$
[c'''.1] $COMP(screams_{v/n}, or_{(v/n)/(v/n,\ v/n)})$
[c'''.2] $COMP(kisses\ John_{v/n}, or_{(v/n)/(v/n, v/n)})$
[d'''] $COMP(Mary\ screams_v, that_{n/v})$
[e'''] $ATTR(violently_{(v/n)/(v/n)}, screams_{v/n})$
Proofs: Def. *"COMP"*, *"ATTR"*, with [a'] - [e'] and [a''] - [e''].

At this point I will consider the special case of sequences $(\alpha^0_{k_o}, \alpha^1_{k_1})$, for which I simply write (α_h, β_k). Note that attribution was restricted to this case by its definition anyway.

DEFINITION of specification:

Let (α_h, β_k) be a sequence of expressions such that $CWB(\alpha, \beta)$.

$$SPEC(\alpha, \beta) \longleftrightarrow_{\text{Def.}} head(\alpha, \beta) = \beta$$

$SPEC(\alpha, \beta)$ is read, "α specifies β in the sequence (α, β)", or "α is specifier, β is specified in the sequence (α, β)".

THEOREM:

$SPEC(\alpha, \beta) \longleftrightarrow COMP(\alpha, \beta)$ or $ATTR(\alpha, \beta)$

Proof: Def. *"COMP"*, *"ATTR"*, *"SPEC"*, *"head"*.

THEOREM:

$COMP(\alpha_h, \beta_k) \longleftrightarrow$ There is a category g such that $g \neq h$ and $k = g/h$.

$ATTR(\alpha_h, \beta_k) \longleftrightarrow h = k/k$.

Proof: Def. *"COMP"*, *"ATTR"*, with $k_0 = h, k_1 = k$. (Where h was used in these definitions, some other variable, e.g. h', should now be substituted to avoid confusion with the h conveniently used here).

EXAMPLES from English:

[a⁺] $SPEC(Mary_n, screams_{v/n})$
[b⁺] $SPEC(John_n, kisses_{(v/n)/n})$
[d⁺] $SPEC(Mary\ screams_v, that_{n/v})$
[e⁺] $SPEC(violently_{(v/n)/(v/n)}, screams_{v/n})$

Proofs: Theorem about *"SPEC"* and *"COMP"*, *"ATTR"*; [a'''], [b'''], [d'''], [e'''].

I made the assumption at the end of the second paragraph of this paper that the notion of "basic serialization" of a given language *L* has been made precise. Let BS_L be the (partial) function from categorially wellbehaved sequences of expressions of *L* to their BS permutations. E.g., drawing on our knowledge of *E* (for English), we have the following orderings, cf. [a$^+$], [b$^+$], [c''] - [c'''], [d$^+$], [e$^+$]:[13]

[a^{++}] $BS_E(Mary_n, screams_{v/n}) = [Mary, screams]_v$
[b^{++}] $BS_E(John_n, kisses_{(v/n)/n}) = [kisses, John]_{v/n}$
[c^{++}] $BS_E(screams_{v/n}, or_{(v/n)/(v/n, v/n)}, kisses\ John_{v/n}) = [screams, or, kisses\ John]_{v/n}$
[d^{++}] $BS_E(Mary\ screams_v, that_{n/v}) = [that, Mary\ screams]_n$
[e^{++}] $BS_E(violently_{(v/n)/(v/n)}, screams_{v/n}) = [screams, violently]_{v/n}$

DEFINITION of basic prespecification and postspecification:

Let (α_h, β_k) be as in the definition of specification, and let $SPEC(\alpha, \beta)$.

$PRE\text{-}SPEC(\alpha, \beta) \leftrightarrow_{Def.} BS_L(\alpha, \beta) = [\alpha, \beta]$
$POST\text{-}SPEC(\alpha, \beta) \leftrightarrow_{Def} BS_L(\alpha, \beta) = [\beta, \alpha]$

EXAMPLES from English:

[a^{+++}] $PRE\text{-}SPEC(Mary_n, screams_{v/n})$
[b^{+++}] $POST\text{-}SPEC(John_n, kisses_{(v/n)/n})$
[d^{+++}] $POST\text{-}SPEC(Mary\ screams_v, that_{n/v})$
[e^{+++}] $POST\text{-}SPEC(\mathrm{violently}_{(v/n)/(v/n)}, screams_{v/n})$

Proofs: Def. *"PRE-SPEC"*, *"POST-SPEC"*, [a$^+$-e$^+$], [a^{++}-e^{++}].

DEFINITION of consistent basic serialization:

PRE-CBS(L) $\leftrightarrow_{Def.}$ For all expressions α, β of *L*, $SPEC(\alpha, \beta) \rightarrow PRE\text{-}SPEC(\alpha, \beta)$

POST-CBS(L) $\leftrightarrow_{Def.}$ For all expressions α, β of *L*, $SPEC(\alpha, \beta) \rightarrow POST\text{-}SPEC(\alpha, \beta)$

CBS(L) $\leftrightarrow_{Def.}$ *PRE-CBS(L)* or *POST-CBS(L)*

PRE-CBS(L) [*POST-CBS(L)*] is read, "*L* is a consistently prespecifying [postspecifying] language" or "*L* has consistent prespecifying [postspecifying] basic serialization" or "*L* has prespecifying [postspecifying] *CBS*".

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[13] Cf. note 12. I use square brackets to emphasize that the order of the expressions in a sequence has been determined by a serialization rule.

CBS(L) is read, "*L* has consistent basic serialization" or "*L* has *CBS*".

FIRST EXAMPLE (with *E* for *English*):

not *CBS(E)*
Proof: Def. "*CBS*", $[\mathrm{a}^{+++}]$, $[\mathrm{b}^{+++}]$.

SECOND EXAMPLE (with *J* for *Japanese*):

CBS(J)
viz. *PRE-CBS(J)*

An assertion such as *PRE-CBS(J)* constitutes an empirical hypothesis about a natural language, consisting of an infinite set of assertions about the BS of pairs of expressions, each of which must in principle be tested by submitting it to the judgement of native speakers. As such, it cannot be "proved". All one can do is test the hypothesis against a significant set of examples to show that the hypothesis is plausible, and then hope that no-one will falsify it. Falsification would, of course, consist in the presentation of a single pair of expressions (α, β) such that *POST-SPEC*(α, β). To date, no such pair has become known.

ILLUSTRATION OF *PRE-CBS(J)*:

Sample lexicon:[14]

NOUN PHRASES

Mary_n	'Mary'
John_n	'John'
$\mathit{Tookyoo}_n$	'Tokyo'

NOUN

hon_c	'book'

VERBS

butta_v	'hit (past)'
omotta_v	'thought'
itta_v	'went'
kaita_v	'wrote'

---

[14] The category assignments may be questionable. E.g., one may have reasons to consider subject and object expressions not as adverbials but as specially marked noun phrases. This would change the status of *ga* and *o*, and would require the following assignment of the verbs: $\mathit{butta}_{(v/n)/n}$, $\mathit{itta}_{v/n}$, $\mathit{kaita}_{(v/n)/n}$. Also one may have reasons to consider the complement of *omotta* as a nominal rather than adverbial expression. This would require the following assignments: $\mathit{omotta}_{(v/n)/n}$, $\mathit{to}_{n/v}$. Neither of these changes, nor any other reasonable recategorization of the basic expressions, will affect the hypothesis about Japanese BS tested here.

ADNOMINALS

$\mathit{omosiroi}_{c/c}$	'interesting'
$\mathit{takai}_{c/c}$	'high'
$\mathit{ookii}_{c/c}$	'large'
$\mathit{no}_{(c/c)/n}$	'of'

ADVERBIALS

$\mathit{saikin}_{v/v}$	'recently'
$\mathit{o}_{(v/v)/n}$	(object marker)
$\mathit{ga}_{(v/v)/n}$	(subject marker)
$\mathit{ni}_{(v/v)/n}$	'to'
$\mathit{to}_{(v/v)/n}$	'with'
$\mathit{to}_{(v/v)/v}$	'that (conjunction)'

AD-ADNOMINALS

$\mathit{amari}_{(c/c)/(c/c)}$	'too'
$\mathit{yori}_{((c/c)/(c/c))/n}$	'than'

AD-ADVERBIAL

$\mathit{goku}_{(v/v)/(v/v)}$	'very'

In what follows I will for convenience always write the specifier to the left of the specified; and I will leave the proof of specification to the reader. Using the definitions of *PRE-CBS* and *PRE-SPEC*, we can then write the hypothesis as follows:

> For all pairs of expressions (α, β) of Japanese with $\mathit{SPEC}(\alpha, \beta)$,
>
> $$\mathit{BS}_J(\alpha, \beta) = [\alpha, \beta].$$

Let us test this by deriving some complex Japanese expressions:

(a) 'Mary hit John'

$\mathit{BS}_J(\mathit{John}_n, \mathit{o}_{(v/v)/n}) = [\mathit{John}, \mathit{o}]_{v/v}$

$\mathit{BS}_J(\mathit{John\ o}_{v/v}, \mathit{butta}_v) = [\mathit{John\ o}, \mathit{butta}]_v$

$\mathit{BS}_J(\mathit{Mary}_n, \mathit{ga}_{(v/v)/n}) = [\mathit{Mary}, \mathit{ga}]_{v/v}$

$\mathit{BS}_J(\mathit{Mary\ ga}_{v/v}, \mathit{John\ o\ butta}_v) = [\mathit{Mary\ ga}, \mathit{John\ o\ butta}]_v$

(a′) $\mathit{Mary\ ga\ John\ o\ butta}_v$

It turns out that (a′) is a correct Japanese sentence radical with BS, and that it translates (a). By applying (a′) to an element *DEC* of the category *dec/v* (a special intonation contour), a declarative sentence is derived, exactly as in English.

(b) '(he) thought that Mary hit John'

$BS_J(\textit{Mary ga John o butta}_v, \textit{to}_{(v/v)/v}) = [\textit{Mary ga John o butta, to}]_{v/v}$

$BS_J(\textit{Mary ga John o butta to}_{v/v}, \textit{omotta}_v) = [\textit{Mary ga John o butta to, omotta}]_v$

(b′) $\textit{Mary ga John o butta to omotta}_v$

(c) 'Mary went to Tokyo with John'

On the pattern of the derivation of $\textit{John o}_{v/v}$ and $\textit{Mary ga}_{v/v}$ in the derivation of (a′), we derive $\textit{Tookyoo ni}_{v/v}$ and $\textit{John to}_{v/v}$; applying the first adverbial to $\textit{itta}_v$, then the next adverbial to the result of the first application, etc., we receive (c′):

(c′) $\textit{Mary ga John to Tookyoo ni itta}_v$

(d) 'interesting book'

$BS_J(\textit{omosiroi}_{c/c}, \textit{hon}_c) = [\textit{omosiroi, hon}]_c$

(d′) $\textit{omosiroi hon}_c$

(e) 'book which Mary wrote'

$BS_J(\textit{Mary ga}_{v/v}, \textit{kaita}_v) = [\textit{Mary ga, kaita}]_v$

Japanese has no special relative clause marker. Therefore, at this stage of the derivation, we will either be able to use a syntactic rule that allows us to use any expression of the category v in the category c/c, or we will have at our disposal an element *REL* of the category $(c/c)/v$, morphologically realized as zero (but possibly manifest in a special intonation). Which way it goes depends on the manner the phenomenon of zero marking is treated in the grammar and need not worry us here. In any event, we will move $\textit{Mary ga kaita}_v$ to the category c/c.

$BS_J(\textit{Mary ga kaita}_{c/c}, \textit{hon}_c) = [\textit{Mary ga kaita, hon}]_c$

(e′) $\textit{Mary ga kaita hon}_c$

(f) 'John's book', 'book of John('s)'

$BS_J(\textit{John}_n, \textit{no}_{(c/c)/n}) = [\textit{John, no}]_{c/c}$

$BS_J(\textit{John no}_{c/c}, \textit{hon}_c) = [\textit{John no, hon}]_c$

(f′) $\textit{John no hon}_c$

(g) 'too high'

$BS_J(\textit{amari}_{(c/c)/(c/c)}, \textit{takai}_{c/c}) = [\textit{amari, takai}]_{c/c}$

(g′) $\textit{amari takai}_{c/c}$

(h) 'larger than Tokyo'[15]

---

[15] No special comparative marker needs to be used if a *yori*-expression is applied to the adjective.

$BS_J(Tookyoo_n, yori_{((c/c)/(c/c))/n}) = [Tookyoo, yori]_{(c/c)/(c/c)}$
$BS_J(Tookyoo\ yori_{(c/c)/(c/c)}, ookii_{c/c}) = [Tookyoo\ yori, ookii]_{c/c}$

(h′) *Tookyoo yori ookii*$_{c/c}$

(i) 'very recently'
$BS_J(goku_{(v/v)/(v/v)}, saikin_{v/v}) = [goku, saikin]_{v/v}$

(i′) *goku saikin*$_{v/v}$

All the complex expressions derived are proper Japanese expressions in BS and thus lend support to the hypothesis, *PRE-CBS(J)*.

The preceding discussion shows how the definition of specification in turn permits a precise definition of the intuitive concept of consistent basic serialization. One need not stop here, of course. On the one hand, one may relax the definition of *CBS* so as to admit "consistent SVX languages", as follows:

DEFINITION of a language with consistent basic SVX serialization:

CB-SVX(L) $\longleftrightarrow_{\text{Def.}}$ For all expressions α_h, β_k of L,

$$SPEC(\alpha, \beta) \rightarrow \begin{cases} PRE\text{-}SPEC(\alpha, \beta) \text{ if } h = n \text{ and } k = v/n \\ POST\text{-}SPEC(\alpha, \beta) \text{ otherwise} \end{cases}$$

One can see how deviation of a language L from CBS complicates the formulation of BS_L. E.g., for English, which is an inconsistent SVX language, one needs to distinguish further categories and even subcategories[16] as well as information concerning the internal structure of expressions in the formulation of BS_E. Inconsistency is thus directly reflected as rule complexity; and languages with CBS are distinguished from all others by having a single BS rule without special cases, viz. one of the following two rules:

For all α, β of L, $SPEC(\alpha, \beta) \rightarrow PRE\text{-}SPEC(\alpha, \beta)$.
For all α, β of L, $SPEC(\alpha, \beta) \rightarrow POST\text{-}SPEC(\alpha, \beta)$.

On the other hand, the system developed here is so general that it can be expanded. E.g., I have here defined CBS by means of the BS of pairs of expressions in a specifying relation. One may consider this concept not restrictive enough and rename it "specificational *CBS*" or the like, reserving the term *CBS* for those languages in which, furthermore, complements in non-specifying relationships too either all precede or all follow their heads:

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[16] Subcategorization is needed independently of the problem of basic serialization; cf. Bartsch 1975.

DEFINITION of a more restrictive concept of CBS (*CBS'*):

> $PRE\text{-}CBS'(L) \longleftrightarrow_{\text{Def.}}$ For all sequences F of expressions of L with $CWB(F)$, for all $\alpha, \beta \in F$ such that $\alpha \neq \beta$ and $\beta = head(F)$, α precedes β in $BS_L(F)$.
>
> $POST\text{-}CBS'(L) \longleftrightarrow_{\text{Def.}}$ For all sequences F of expressions of L with $CWB(F)$, for all $\alpha, \beta \in F$ such that $\alpha \neq \beta$ and $\beta = head(F)$, α follows β in $BS_L(F)$.
>
> $CBS'(L) \longleftrightarrow_{\text{Def.}} PRE\text{-}CBS'(L)$ or $POST\text{-}CBS'(L)$

This redefinition may be justified by pointing to the typological correlation of specification with the placement of coordinating conjunctions: in prespecifying languages these conjunctions are usually postposed, in postspecifying languages, preposed; often, however, the conjunction is spread to all conjuncts, and then deleted except for the last occurrence.[17] Let $CWB(\alpha^0, \alpha^1, \alpha^2, \ldots, \alpha^m)$ and, without loss of generality, let α^0 be the conjunction.

In pre-specifying languages:

$[\alpha^1, \alpha^2, \ldots, \alpha^m, \alpha^0]$
or $[\alpha^1, \alpha^0, \alpha^2, \alpha^0, \ldots, \alpha^m, \alpha^0]$

In post-specifying languages:

$[\alpha^0, \alpha^1, \alpha^2, \ldots, \alpha^m]$
or $[\alpha^0, \alpha^1, \alpha^0, \alpha^2, \ldots, \alpha^0, \alpha^m]$
or $[\alpha^1, \alpha^2, \ldots, \alpha^{m-1}, \alpha^0, \alpha^m]$

The more comprehensive definition thus accommodates this correlation in a natural fashion.

One may also expand the set of basic categories considered to include such sentential categories as *dec, imp*, and *int*, leaving all subsequent definitions unchanged. This would yield satisfactory results in certain cases. E.g., in Japanese a question is formed by adding *ka* to a sentence radical: $ka \in B_{int/v}$.

(j) 'Did Mary hit John?'
$BS_J(Mary\ ga\ John\ o\ butta_v, ka_{int/v}) = [Mary\ ga\ John\ o\ butta, ka]_{int}$
(j′) *Mary ga John o butta* ka_{int}

Since this is a correct Japanese question with BS, Japanese even passes this more severe test for CBS. In general, however, such a restrictive definition of CBS may turn out to be useless because of the frequency of sentence-initial adsententials ($\in P_{dec/dec}, P_{int/int}$, etc.) and sentence-final sentential bases ($\in P_{dec/v}, P_{int/v}$, etc.) in postspecifying languages.

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[17] Cf. the Bhartṛhari verse *(dhik) tāṁ ca taṁ ca madanaṁ ca imāṁ ca māṁ ca* and the sentence *mitraṁ (huve) varuṇaṁ ca* to their translations *(alas about)* [*both*] *her and him and Amor and this one and me* and *(I invoke) Mitra and Varuṇa*. Cf. also the change in Greek from *α(-te) β-te* to *(kai) α kai β*, and in Latin-French from *α(-que) β-que* to *(et) α et β*.

In conclusion, I would like to point out that the definitions given here need not be changed to accommodate subcategories. Consider, e.g., the case of modal and auxiliary verbs with non-finite complements. The category v and its descendants, v/n, $(v/n)/n$, $v/(c/c)$, etc., all index finite verbs only, while non-finite verb forms, such as infinitives and participles, belong to other categories, e.g. n and c/c.

EXAMPLES from English:

$$SPEC(singing_{c/c}, is_{(v/n)/(c/c)})$$
$$SPEC(go_{n}, can_{(v/n)/n})$$

Their basic serialization agrees with the general postspecifying character of English:

$$BS_E(singing_{c/c}, is_{(v/n)/(c/c)}) = [is, singing]_{v/n}$$
$$BS_E(go_{n}, can_{(v/n)/n}) = [can, go]_{v/n}$$

The assignments of participles and infinitives appear less arbitrary in flexional languages than in English.[18] Whatever the assignment, if it is carried out properly the non-finite verb forms will turn out to be specifiers, viz. complements, of their modals and auxiliaries.

Consider, finally, expressions that may be assigned several categories. The analysis of such expressions requires special care because they may sometimes be constructed as specifier, sometimes as specified. A case in point is the group of "particles" such as *in, up, along* in English and their analogs in other languages (e.g. the so-called preverbs of Proto-Indo-European). These can function as adverbials and as adpositions, e.g.:

$$SPEC(in_{(v/n)/(v/n)}, comes_{v/n})$$
$$SPEC(Stanford_{n}, in_{((v/n)/(v/n))/n})$$

English reflects this difference in the serialization of such expressions, which is postspecifying in both cases:

$$BS_E\ (in_{(v/n)/(v/n)}, comes_{v/n}) = [comes, in]_{v/n}$$
$$BS_E\ (Stanford_{n}, in_{((v/n)/(v/n))/n}) = [in, Stanford]_{(v/n)/(v/n)}$$

---

[18] E.g., the assignment of n to infinitives such as *gehen* in such German expressions as *kann gehen* 'can go' is well motivated: German has a special morphology to distinguish infinitives from stems (stem *geh-*, inflexion *-en*); infinitives can be used in typical noun phrase positions as the subject position: *Gehen ist gesund* 'walking is healthy'; and they pronominalize easily: *Ist Gehen gesund? Ja, es ist gesund* 'it is healthy'; *Kann er gehen? Ja, er kann es* 'he can (do) it'.

Such examples show that the definitions developed here on the evidence of obvious examples of attribution and complementation cover less obvious cases in a unique and satisfactory fashion and may thus serve as a standard of adequacy in the grammatical and typological analysis of languages.[19]

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[19] While the notion of specification (modification, qualification, *détermination, Unterscheidung*) remained a vague, semantically based notion in earlier studies of the problem, it was recognized in Bartsch and Vennemann (1972) that it has, in addition to semantic properties, the syntactic property of what I later called categorial constancy. Yet it was also said there that "the principle of natural serialization [which says, in the terminology of the present paper, that an ideal natural language has *CBS*] . . . can be made explicit only in a theory of grammar which represents semantics in an explicit way" (Bartsch and Vennemann (1972:131). I have demonstrated in the present paper that the concepts of specification and of CBS (and consequently of "natural serialization") can be defined in purely syntactic terms.

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